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PRINCIPLES

OF

MEDICAL SCIENCE

AND

PRACTICE.

PART I.

PHYSIOLOGY.

BY

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> To discover new parts has been a principal object in the researches of the young and practical Anatomist; but the connexion, arrangement, mode of action, and uses, of the whole or of particular organs, have more commonly been reserved for the consideration of those whose views were extended further.—*Mr. John Hunter.*

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PREFACE.

THE objects of the following work are to give a systematic view of the actions of the body in health and in disease; to trace these actions to their causes and effects; to ascertain in what manner the healthy action of one organ contributes to the healthy action of another, the diseased action of one organ to the diseased action of another, and the diseased action of one organ to the healthy action of another; to point out the connection between the natural and artificial cure of diseases, and ſrom this connection to deduce certain principles, which may give rationality to our practice, and unite the experience of empiricism with the theory of science. To record the opinions of others upon this wide and extended subject, would require a longer life than is usually allotted to man, and more leisure than is consistent with my professional avocations; and from such a medley of ideas to mould the form and dimensions of a rational practice, would seem to require little less than that super-human power, which desired the chaotic mass to unite in those proportions which constitute what we call the world, which said—*Let there be light and there was light.* Having formed opinions (connected more particularly with the physiology of the nervous system, and with the agency of the atmosphere upon living bodies) which are in a great degree peculiar to myself, and which have an apparent tendency to simplify our views of the animal economy, I shall, without farther preamble, submit these opinions to the consideration of my professional brethren. The fair, candid, and manly criticism of a mind anxious only to promote the cause of science, and of medical science in particular, is the great bulwark of our profession. To such criticism I most willingly submit opinions, which, if correct, ought not to be hoarded, and which, if proved to be erroneous, shall be abandoned. Having fully stated my case, and having ascertained the objections which will be made by my learned friend on the other side of the question, I shall avail myself of the privilege of the Bar, and offer some observations in reply; but such reply will be purposely delayed until *the whole of these objections* can be concentrated into a tangible form, and subjected, in their turn, to the ordeal of criticism.

HARDWICKE SHUTE.

GLOUCESTER, June 20, 1824.

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ERRATA.

Page	*Line*	
5,	13,	after *not* dele comma.
5,	14,	after *circumstances* dele comma.
9,	24,	after *sources* adde comma.
9,	25,	after *breathe* adde semicolon.
24,	20,	for *who* read *which*.
24,	28,	for *who* read *which*.
41,	1,	for *who* read *which*.
57,	20,	for *who* read *which*.
72,	8,	for *prevents and cures* read *prevent and cure*.
72,	25,	for *variolus* read *variolous*.
73,	4,	for *variolus* read *variolous*.
77,	13,	for *left internal and jugular veins* read *left internal jugular and subclavian vein*.
287,	3,	for *assimilative* read *digestive*.
292,	10,	for *life-action*; read *life*; *action*.
346,	22,	for *whom* read *which*.
397,	25,	for *peu* read *peut*.

CHAPTER I.

Preliminary Observations.

THE animal frame is composed of certain elementary substances; these, when united in certain proportions, constitute the solids and fluids of the body, and when moulded into certain forms, are denominated organs. These organs, in consequence of the peculiar arrangement of their elementary substances, are, under certain circumstances, endowed with the power of performing certain actions; and that this power or property of an organ is connected with its organization, we infer from the acknowledged fact, that the effects produced by different organs, always correspond with the different arrangement of their elementary substances; in other words, with their peculiar organization. The stomach, for instance, does not, under any circumstances, possess the power of elaborating bile, or the liver of secreting the gastric fluid; the peculiar organization therefore of a part, is essential to the existence of its peculiar action, and it follows that every action of the body is, strictly speaking, organic. But it is only under certain circumstances, that the several organs are capable of performing their appropriate functions; that these circumstances, which influence the action of an organ, are not altogether dependent upon its organization or peculiar arrangement of its elementary substances, has been inferred from the fact, that a diminution, or even loss of power in an organ, is not always accompanied with a corresponding change in its organization sufficient to account for the phenomenon observed; thus there is no apparent difference in the structure of an organ, which continues sensible to its appropriate stimulus, or which has lost its power of being excited into action; at least, there is no apparent difference sufficient to enable us to say, why the organ is endowed with, or deprived of, its susceptibility of action. It seems, therefore, that under these different circumstances, the organ must have possessed, or have been deprived of some property, which does not apparently belong to its organization.* This imaginary property has been denominated the principle of life; when deprived of this principle, no organ, under any circumstances, is capable of performing its appropriate function, and therefore every action of the body is, strictly speaking, a vital action; in other words, dependent upon the living principle. To say, therefore, that any action of the body is solely vital, or solely organic, that is solely dependent upon the living principle as unconnected with organization, or solely dependent upon organization as unconnected with the living principle, is not consistent with fact; a peculiar living organization, is essential to the existence of a peculiar living action; no action can be organic, which is not vital, and no action can be vital, which is not, at the same time, organic. Physiologists have usually divided the several actions of the system into three different classes, denominated the natural, the vital, and the animal functions; but it is clear that every action of the body is equally natural, equally vital, and equally characteristic of the animal matter to which it belongs. The arrangement appears to be also objectionable, by conveying the idea, that these different functions are essentially different from each other. This, however, was not the object intended; it was founded upon some peculiarities which characterize, not the actions themselves, but the effects produced by them; and I am of opinion that many important advantages have been derived from the classification. Modern Physiologists have arranged the different actions of the body under two separate classes—the animal and the organic functions—but every action of the body is equally organic, and equally characteristic of the animal matter to which it belongs. This arrangement is expressly founded upon the idea, that these different functions are essentially different from each other; that is, different not only in their effects, but in the mode of action by which these effects are produced. The French physiologists have gone so far as to imagine, that the organs subservient to these different functions, are endowed with a different species of life: the first has been denominated animal, or exterior life, that is connected with objects not forming a part of the body itself; the last, organic, or interior life, that is wholly dependent upon the organization of the system. There is no one action of the body which is not immediately or remotely connected with, and dependent upon, objects not constituting a part of the body itself;—there is no one action of the body, which is not immediately or remotely dependent, upon the food we take, and the air we inspire. Every action of the body is, strictly speaking, organic; and the organization, as well as the action of the system, are dependent upon objects exterior to the body itself; it is clear, therefore, that the organic, or interior life, is not, independent of circumstances, exterior to the body: the animal, or exterior life, and the organic, or interior life, are equally dependent upon the organization of the system.—This physiological arrangement is generally attributed to Xavier Bichât.—I leave it for those who are anxious to trace ideas to their origin, to determine what constitutes the difference between the "sensible and irritable fibre of Dr. Darwin," the "sensation with and without consciousness" of Dr. Monro, the "mental and corporeal actions" of more unprofessional writers, and the "animal and organic functions" of the French physiologist. There are strong reasons for supposing, that Bichât was more particularly anticipated by our own countryman, Dr. Wilson Philip; that writer says, " organic contractility," that is, vital excitability, has no relation to a centre, but exists in every part, independently of its existence in any other; while animal contractility, that is, animal excitability, in every part depends upon the existence of a common centre."* It is clear that the animal excitability must be vital, because, without a peculiar living organization it cannot exist; and the vital excitability must be characteristic of the animal matter to which it belongs, because, without a peculiar animal organization, it cannot exist.† The object of language, however, is to convey ideas; and every author has, I conceive, a right to expect, that the term he adopts shall be accepted according to the definition he has given to it. If my objections were confined to the expressions only, and did not apply to the ideas which they were meant to convey, I might be accused of shewing a much greater disposition to cavil and find fault unnecessarily, than to advocate the cause of truth; but the whole theory appears to me to have been erected upon a very insecure basis. It still remains to be proved, "that the brain does not exercise a direct controul over the motions of the heart, and other muscles of involuntary motion;" and therefore it still remains to be proved, that "the organic contractility, that is, vital excitability, has no relation to a centre, but exists in every part, independently of its existence in any other." The arrangement proposed by Bichât is not precisely similar to that of Dr. Wilson Philip, but appears to be founded upon the same assumption. As the supporters of this opinion have been under the necessity of advancing the absurd doctrine, that the nerves of the heart are absolutely useless; and as I shall have occasion, in a future part of these observations, to advert to the modification of this doctrine proposed by Dr. Wilson Philip, and to give my reasons for dissenting from it, I shall for the present briefly express, what I conceive to be the fundamental error of the French physiologist alluded to:—"ceux qui comme Fontana ont rejetès formellement toute intervention de la puissance nerveuse, ont été forcès d'admettre, que les nerfs, destinès partout ailleurs a porter la vie le sentiment et le mouvement, n'avoient dans le cœur aucun usage commun."* Dr. Jacques Moreau has denominated the organic life of Bichât "La vie de nutrition." I am disposed to think, that almost every function of the body is employed in one extended system of nutrition, the conversion of the food we take, and the air we breathe, into the component parts of the animal frame; but I do not adopt the arrangement proposed by Dr. Moreau, because it is founded upon the same questionable fact, as that of the writers already alluded to. Every action of the body I consider to be, under the same circumstances, equally referable to a centre—in other words, to the brain; and therefore I do not imagine, that the different functions of the body are obedient to different laws. It is true that some actions may be more immediately referable to a centre, that is, more immediately dependent upon the brain than others; but every organ endowed with the power of action is also endowed with nerves; and therefore I conceive, that every action of the body is, to a certain extent, dependent upon its nerves, and, if so, remotely dependent upon the brain. There are several functions of the body in which the mind takes no part; there are others, which cannot exist without it. The first have been denominated corporeal, the last mental functions. I adopt this arrangement as being more simple, and, I may say, more generally understood, than the organic and animal functions of modern physiologists; and I adopt it upon the express supposition, that the actions of the mind and of the body, are not essentially different from each other. It may be said, that the actions of the mind are, to a certain extent, corporeal; but the actions of the body are not mental, and therefore the distinction may be clearly defined. Those actions of the body, in which the mind does not necessarily take part, I consider to be corporeal, and all others mental. Those actions of the body, in which the mind does not necessarily take part, are all directed to the organization of the system. The support of the animal frame is evidently dependent upon two sources; the food we take, and the air we breathe, and I shall endeavour to shew, that these are converted to the uses of the body, by constituting component parts of the animal frame. Certain organs of the body are evidently employed in converting the food we take into a component part of the system: the actions of these several organs, are physiologically arranged under the titles of digestion, absorption, assimilation, circulation, and nutrition. Other organs are evidently concerned in converting the air we breathe to the purposes of the animal economy, and, I believe, into a component part of the animal frame. The actions of these organs, are physiologically arranged under the separate titles of respiration, circulation, and, probably, nervous action. But there are certain parts of our food, and also of the animal frame, which were originally, or have become excrementitious, that is, useless to the system; certain organs of the body are employed in removing these from the system, and the actions of these organs, have been arranged under the titles of secretion and excretion, so, that the different processes of digestion, absorption, assimilation, respiration, circulation, nervous action, nutrition, secretion, and excretion, may all be considered as constituting one extended system of actions, by which certain parts of the food we take, and the air we inspire, are converted into the component parts of the animal frame, and by which others, not calculated to enter into this composition, are eliminated from it. The sexual functions are but little understood, but they are partly included in the several processes already mentioned, and may be arranged under the general title of generation, in which we include those several processes, by which the food we take, and the air we breathe, are ultimately converted into the component parts of a separate individual. We find that a constant supply of air, through the medium of the respiratory organs, is essential to the continued actions of the system ; if this supply be intercepted even for a short time, the several organs are deprived of their power of action. It seems, therefore, that our food contributes more immediately to the organization, and the air we breathe to the action of the system. But I have already said, that the action of an organ is dependent upon its organization, and therefore cannot be supposed to imply, that our food is not remotely concerned in supporting the living actions of the system. These several actions appear to be more immediately dependent upon the supply of air than of food ; but I shall endeavour to shew, that the air does not contribute to the support of living action, unless it unites with and becomes a component part of the living organization. The process of digestion consists in converting certain parts of our food into chyle, and in removing from the system those parts which are not calculated to enter into its composition. But the object of our taking food, is not its conversion into chyle, or even into blood; it is intended to constitute a component part of the animal frame. If it shall appear, that the conversion of certain parts of the blood into the component parts of the body, is accomplished by a process similar to that by which certain parts of our food are converted into chyle, I shall be justified in saying, that the process of digestion is not, strictly speaking, accomplished, until certain parts of our food are converted into the component parts of the animal frame; and I shall endeavour to shew, that the different functions of digestion, absorption, assimilation, circulation, nutrition, secretion, and excretion, all constitute one extended system of actions, by which our food is, in reality, digested. If we consider the process of digestion to consist, not only in converting certain parts of our food into chyle, but in removing from the system those parts which are not calculated to enter into its composition, we shall find that the changes which the blood undergoes, are precisely similar in object and effect, to those which take place in the stomach; and upon these grounds I have ventured to adopt the unusual expression, that the blood is digested. Respiration is certainly one of the most important agents in the animal economy; it is generally considered as connected with the action, rather than with the organization of the system; but our reasonings upon this subject appear to have been involved in much obscurity, by our not attending, to what I conceive tò be the fact, that the air we inspire is no more capable of supporting the actions of the system, except it be incorporated with it, than the food we take is capable of contributing to its organization, without being digested. That the influence of the air does unite with, and form a component part of the living organization, I infer from the acknowledged fact, that the action of the heart, which is admitted to be immediately dependent upon this influence, may be excited for a certain time, after it has been absolutely removed from the system. If we admit that the arterial influence is necessary to the action of the heart, how shall we account for the fact, that this action may be excited long after the supply of this influence has been intercepted, without admitting that it must have united with, and become a component part of the organ itself? If the change which the air undergoes in the lungs, be precisely similar to that which the food undergoes in the stomach, I am justified in adopting the unusual expression, that the air is digested. The actions of the system are dependent upon a living organization; if the aerial influence cease to constitute a component part of that organization, I conceive that that organization ceases to be endowed with the principle of life, and therefore ceases to be endowed with the susceptibility of action; the organization is imperfect, and therefore that property which belongs to a perfect organization only, cannot exist. The air, like our food, must, I conceive, be digested, and conveyed to the several parts of the system, for the purpose of being incorporated with it. I shall, consistently with this view of my subject, first endeavour to point out, in what manner the several processes of digestion, absorption, assimilation, respiration, circulation, nutrition, secretion, excretion, and, probably, nervous action, contribute to the healthy organization of the system, and give my reasons for considering them as one extended system of actions, by which the food we take, and the air we breathe, are digested; and then consider what are the properties of the organization itself. My subject naturally divides itself into two distinct portions—the organization, and the living action of the system; and the latter may be still further divided into the corporeal and mental functions; the connexion between these functions and the living principle, will involve some few observations upon the nature of what we call Life.

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* It will be necessary for my Reader to bear in mind the import of the above expressions—" no apparent difference," " do not apparently belong."

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* Ed. Med. & Surg. Journ.

† The similarity, or points of resemblance, between certain animal and vegetable functions, are not sufficient to justify us in assuming their identity; the term animal excitability cannot be used as a mark of distinction, without implying that vital excitability is not animal; in other words, without implying that it is peculiar to vegetable, and does not belong to animal matter.

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* Le Gallois.

CHAPTER II.

ORGANIZATION—DIGESTION.

THE conversion of alimentary matter into chyle, is considered to be the peculiar and appropriate function of the digestive organs. In most animals we discover a stomach, or some enlargement of the alimentary canal, which is evidently calculated to retard the progress of the food; on this account the process of digestion has, from the earliest dawn of physiological inquiry, been attributed to the action of that organ; but the addition of certain important fluids to alimentary matter, immediately after it has passed the stomach, and the situation of those vessels which convey the product of digestion to the several parts of the system, have led to the generally received opinion, that the process of digestion is not completed in the stomach. The change which the food undergoes in this organ has been attributed to heat, trituration, putrefaction, fermentation, admixture of the nervous fluid, and even to the agency of the soul itself. Vallisneri, according to Mr. John Hunter, or, in the opinion of others, Cheselden first suggested the idea, that the fluid secreted by the stomach, might be a solvent *sui generis*, and this opinion has been confirmed by the observations and experiments of Reamur, Stevens, Spallanzani, and others. But these experiments, though decisive as to the important agency of that fluid, in the process of digestion, seem to have diverted our attention from those important changes which the food undergoes, before it is subjected to the action of this menstruum.

A modern writer* says that, "the animal frame is supported by aliment taken into the stomach, where it undergoes the *first* process of digestion." It is, I conceive, a fact of practical importance, and not always sufficiently attended to, that digestion begins with the teeth, and the consideration of this subject appears to commence still farther back. The first question which naturally suggests itself is this—Does all matter, which is capable of being converted into chyle, and subsequently into blood, and the component parts of the animal frame, contain in itself those ingredients which constitute animal matter. There is no apparent resemblance between, a chestnut, a potato, a piece of bread, and animal matter; there is no ingredient in these substances, which can be said to be of an animal nature; and therefore we answer the question in the negative. Do these substances contain in themselves the principles of the various ingredients, which constitute chyle, blood, and animal matter? It seems necessary to admit, that this must be the case, although we are unable positively to demonstrate the fact. If alimentary matter be the only source of blood, it follows that alimentary matter must contain in itself, either the ingredients themselves which are found to constitute blood, or the principles from which those ingredients are formed. Are there any other substances, which uniting with our food, may contribute to the organization of chyle, blood, and animal matter? There are none, the air excepted, which are not derived from the blood itself; and these cannot possibly have contributed to its original formation. The analysis of animal matter has satisfactorily proved, that it consists in a great degree of various substances, which are known to constitute a part of the mineral kingdom. Dr. Henry says,

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* Dr. Barlow.

"by the long-continued action of hot water upon bones, we form a solution which separates spontaneously into two distinct substances—fat and gelatine; while the *earthy* ingredients remain undissolved."* But fat and gelatine are not elementary principles; they are capable of being still farther decomposed, and may be proved to contain in themselves, mineral or *earthy* substances. This observation may be applied more particularly to gelatine. "After incineration a coal remains, which contains a minute proportion of phosphate of soda and phosphate of lime."† Animal matter then, consists of certain *earthy* ingredients, and of other substances which have been denominated primary animal compounds; and these compounds contain in themselves a certain portion of mineral or *earthy* substances. Whence are these substances derived? The food of a great variety, I might say a great proportion of animals, consists entirely of grass or vegetable matter; and we have the authority of Haller for saying, that human life has been supported by grass alone. "Puer in deserto loco derelictus a matre, educatus inter oves et capras, didicit gramine vesci: atque cum captus esset, seligere inter oblatas plantas, eas quibus oves delectantur, annotatus est."[*] Whole nations are said to live upon the beech and chestnut. "Imo vero prisci veteris Græciæ coloni, vescebantur solâ fago, sive quercu eduli, quæ nostro etiam ævo, in Hispania et Lusitania mensis magnatum inferuntur."[†] It is also recorded in history, that a large company of soldiers were supported for a considerable period upon gum arabic. We also know, that at this present time, a very large proportion of our own population, is supported upon vegetable matter alone. We see positively, that vegetable is converted into animal matter; how shall we account for the fact? Is it not probable, that animal and vegetable matter consist of the same principles, differently arranged and combined, and that these principles exist in the earth? Thus the grass, the beechnut, the chestnut, the gum arabic, may be supposed to have derived from the earth, and to contain in themselves, the principles of those mineral substances, which originally formed component parts of the earth. These principles may, by the process of *vegetable* digestion, have been enabled to enter into those peculiar combinations, which constitute vegetable matter;—these principles, which constitute vegetable matter, may, by the process of *animal* digestion, be enabled to enter into those peculiar combinations, which constitute animal matter. Dr. Fordyce says, " Between different species of matter, there exist what are called attractions ; and these vary according to the circumstances in which the matter is placed ; so that although there be always present substances capable of being converted into chyle, yet that change will never take place, unless they be influenced by the action or circumstances, which they meet with in the organs of digestion of a living animal."* It seems then, that the different ingredients contained in chyle, blood, or animal matter, and which, in the present imperfect state of chemistry, are denominated elementary substances, because we cannot effect a further decomposition, may, in reality, be compound bodies, consisting of certain principles originally derived from the earth or the surrounding atmosphere, and combined in those peculiar proportions, which constitute animal matter. It has been supposed, that even pure water may contain in itself the elementary principles of animal matter. " Aqua, ex qua Thales, Paracelsus, Helmontius, et

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* Henry's Chemistry.

† Ibidem.

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[*] Prælectiones. [†] Ibidem.

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* Dr. Fordyce on Digestion.

Boyleus, Origines omnium corporum, animantium et vegetantium deducunt."* If alimentary matter contain in itself, not the ingredients but the principles of chyle, the process of digestion by which chyle is formed, must be supposed to consist, in disuniting these principles from their original combinations, so that they may re-combine in those peculiar proportions, which constitute chyle. However various the theories, by which authors have attempted to elucidate the intimate nature of digestion, they all agree as to the point to be explained ;—they all attempt to account for the new combination, which is admitted to have taken place. The fact, that certain substances are contained in the chyle and in the blood, which cannot be discovered in our food, from whence these fluids are derived, does not seem to admit of any other explanation. Dr. Fordyce says, " If we have three species of matter, and take the smallest possible part of each, viz. A, B, C ; A may unite with B, and form a compound—M, which may unite with C, and form a compound particle—X ; or A, combined with C, may form N, which, combined with B, may form a compound particle—Y ; or B and C may form a compound—

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* Prælectiones.

O, which, combined with A, may form a compound particle—Z. In the same manner, if A and B unite to form M, and C and D unite to form N, and if M and N united form X, this compound X may be decomposed, so that A united with D, may form O, while B and C may form P, and these compounds O and P, may not be capable of combining with each other, and thus from X two other compounds may be formed, containing exactly the same matter, and incapable of uniting with each other."* The Abbé Spallanzani concludes from his experiments, "that in the gallinaceous class, trituration and the gastric fluid mutually assist each other, in performing the great function of digestion; the former, by breaking down the aliment, acts as the pre-disposing cause; the latter, when it is thus prepared, penetrates into it, destroys its texture, divides their particles, and disposes them to change their nature, and to become animalized."† Dr. Marcet speaks of the "wonderful chemical powers of the digestive organs;" the "almost instantaneous conversion into fibrine, fatty matter, and red particles, with the constant appearance of certain salts; all these substances bearing a certain proportion to each other;" and concludes by expressing his hope, that " our enlarged views of chemistry, and our improved method of cultivating that science, will soon throw new light upon this important part of physiology.* These different authorities all verge to the same point,—to the opinion which I am endeavouring to support, that " the stomach is the seat of the assimilating power, in which the elements of food and drink, of whatever kind and variety, first separate, and then unite into the only one combination, fit for undergoing the other specific changes."† It might almost appear unnecessary, to quote authorities in support of the opinion so generally entertained, that digestion is, in reality, a disunion and re-combination of certain principles contained in our food: but I wish my reader to have a clear idea of the nature of the " other specific changes," which the food is destined to undergo in its progress through the system.

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* Med. Comment. vol. 17.

† Med. Comment. vol. 10.

The first process of digestion, corresponds with the preparatory step necessary to many chemical combinations, which take place out of the body. Many substances will not act chemically upon each other, except the attraction of cohesion, or that power by which the integral parts of substances are kept together, be first diminished: by the application of heat, for instance; by mechanical means; or by the intervention of a fluid. It is not unusual to combine, the mechanical effects of friction with the agency of a fluid; this process has been termed trituration: the resemblance between the process of mastication, (in which are combined the mechanical effects of chewing and the agency of the saliva,) and trituration are self-evident; and they are both equally to be considered as preparatory steps to those changes, which the different substances acted upon may be destined to undergo. The necessity of this process to easy and perfect digestion may be inferred, from the anxiety evinced by nature to provide organs for the purpose. In those animals, birds for instance, who have no teeth, we find a crop, which is simply a pouch for the purpose of retaining the food, until it is moistened, or probably still further acted upon, by the fluid secreted into that organ; the food then passes on to the gizzard; where it is subjected to an action precisely similar to that, which is accomplished by the teeth of other animals. There are some animals, who live solely upon grass: the mastication necessary to perfect digestion, if performed at the time of feeding, would render it impossible for them to collect the food necessary to their support, without constant and uninterrupted locomotion; but even in these, the process of mastication cannot be dispensed with; and the animal is endowed with the singular power, of subjecting the food to this necessary process in the reclining posture, and at the time most convenient to itself; the food regurgitates into the mouth, and a second mastication, termed chewing of the cud, is performed. Chewing of the cud is not a necessary process in all animals who feed on vegetable matter; but we may observe, that those animals who chew the cud, are almost constantly employed in feeding; from whence it may be inferred, that the nutrient part of the food, is so disproportioned to the exigencies of these animals, that they cannot be supported without a constant supply, which would, but for this provision of nature, involve the necessity of constant locomotion. It may be observed, that the saliva which is constantly passing into the stomach, not only acts as a diluent, but also contains a very considerable portion of atmospheric air. Haller, in speaking of the air, says, " qui magnâ copiâ in ventriculum descendit inclusus vesiculis salivosis; omnis enim deglutitio cum inspiratione conjunctus est, et aër quidem præcedit propulsus a potu, non in asperam solum arteriam, sed in œsophagum etiam descendit, cum nihil impediat."* As all changes in the organization of a substance, appear to be counteracted by the principle of life, it is probable that our food, which is frequently endowed with this principle, may be more disposed to enter into new combinations, when that perfect organization, which is essential to the union of this living principle with matter, has been destroyed in the first process of mastication. Thus seeds, which are evidently endowed with a vital principle or property, are constantly passing through the alimentary canal of animals, without being digested; in consequence, as it would appear, of the first process of mastication having been imperfectly accomplished. "Robustissimus Equus Avenam non manducatam adeò non subigit, ut aliquando visa sit egêsta vim germinandi retinuisse. De avibus notissimum est, eas Semina Baccasque in remota loca sylvasque plantare, quæ elapsa per alvum germinant."† As birds feed principally upon seeds, which have not yet lost their vital principle, a powerful muscular action, appears to be in a peculiar manner necessary to their conversion into chyle; this, as before stated, is accomplished by that peculiar organ, denominated the gizzard; but this powerful action is rendered still more efficient, by the additional mechanical assistance of hard substances, such as small pebbles, which the animal, by a species of instinct, picks up occasionally, in order that they may be conveyed to the gizzard: these may be considered as so many adventitious teeth, performing in the stomach that mastication, which is more frequently effected in the mouths of other animals. It has been said, that these pebbles are picked up by mistake or accident; but it has been ascertained, that the animal will not thrive, if placed in a situation where it cannot acquire them; it was also very justly remarked by Mr. John Hunter to be a singular circumstance, that those animals only who have gizzards, ever commit this error, or are so stupid, as to mistake pebbles for their food. The importance of mastication to easy and perfect digestion, is strongly pointed out by these phenomena of comparative physiology; and there is no instance, in which it does not appear to be a preparatory step to those further changes, which the food is destined to undergo in the stomach.

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* Medico-Chirurg. Transact. vol. 6.

† Dr. Webster.

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* Prælectiones. * Ibidem.

The stomachs of different animals, are usually classed according to their structure, which is either membranous, muscular, or intermediate, that is, partaking of both. Dr. Monro seems to have attributed much importance to the muscular action of this organ. In some animals he considers the muscular action of the gizzard to be the sole cause of digestion. "The digestion of these animals is performed merely by attrition, as is evinced by many experiments. This was put beyond all dispute by the experiments of Mr. Tauvrey, who gave a species of metal to an ostrich, convex on one side, and concave on the other, but carved on both; and on opening that creature's body some time after, it was found, that the carving on the convex side was all obliterated, while the engraved character remained the same as before on the concave side, which was not subjected to the stomach's pressure; which could not have happened, had digestion been performed by a menstruum, or any other way whatsoever, but may be easily solved, by allowing a simple mechanical pressure to take place. We are however, by no means to conclude from this, as some have rashly done, that in the human body, digestion is performed by a simple attrition."* The circumstance here stated, that the fluids of the stomach did not act upon the metal, is no proof, that they may not or do not act upon the natural food of the animal. There seems to be no reason to question the fact, that the gastric fluid is the principal agent in digestion; and we find, (as might have been expected, and has been proved by the experiments of Spallanzani,) that it is different in different animals, and that its solvent power is, in many instances, almost confined to that food, which is natural to the species. This fluid is endowed with certain chemical properties, as may be proved by its effect upon putrid animal matter, and other substances *out* of the body. It has been inferred from this circumstance, that the process of digestion may be wholly chemical; but I have yet to learn, that chyle has ever been produced without the intervention of a living organ; and therefore I conclude that digestion is, in reality, a living process: we are acquainted with the effect produced, with the object to be accomplished; but are wholly ignorant, in what manner, the changes we witness are effected. The gastric fluid has been subjected to chemical analysis; but I am of opinion, that our views of living action have been rather obscured than elucidated, by the presumed identity of chemical and living affinities; and there is nothing in the chemical analysis of the gastric fluid, which has made us, in any respect, better acquainted with the nature of digestion.

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* Monro's Works.

Some writers, (I allude more particularly to the experiments and observations of Dr. Montegre,) have supposed the saliva to be in reality the gastric fluid; but Spallanzani has proved satisfactorily, that nothing like digestion, or the conversion of food into chyle, takes place in the crops of birds; and it is now generally admitted, that the gastric fluid is, as its name imports, a secretion from the stomach. Some authors have attempted to point out the particular parts of the stomach, from which this secretion is derived. Sir Everard Home says, "from the result of these experiments, as well as from the analogy of other animals, it is reasonable to believe, that the glands situated at the termination of the cuticular lining of the œsophagus, which has been described, secrete the solvent liquor which is occasionally poured on the food, so as to be intimately connected with it, before it is removed from the cardiac portion."* This idea is founded upon the opinion, that a kind of hour-glass contraction takes place in the stomach, during the process of digestion. The stomach is imagined to contract upon itself, so as to establish a complete division between its cardiac and pyloric portions, and these portions are supposed to have appropriate and distinct functions to perform. He further imagines, that there is a communication between the cardiac portion of the stomach and the circulation of the blood, through the medium of the spleen; and that the peculiar function of the latter organ, is to convey off the fluids from the cardiac portion. The same author says, "that the food is dissolved in the cardiac portion of the stomach, is *proved*, by that part only being found digested after death." According to the opinion of Mr. John Hunter, the gastric fluid gravitates towards the most dependent part of the stomach, which is, I conceive, its cardiac portion; no *proof* therefore, of the contraction of that organ, can well be inferred from the circumstance mentioned. That the chyle is formed, not in the cardiac, but the pyloric portion of that organ, is said "to be proved, by the result of some experiments of Mr. Hunter's, made upon dogs, in the year 1760. As these experiments were instituted for a very different purpose—that of determining, whether the gastric juice is acid or alkaline—the results were detailed without any possible bias. In all the dogs, the food was least dissolved or even mixed, toward the great end of the stomach, but became more and more so toward the pylorus, and just within the pylorus, it was mixed with a whitish fluid like cream, which was also found in the duodenum."* The observations of Mr. Hunter amount to this; that digestion is a process, not of the cardiac portion of the stomach only, but of its whole structure; but seem to prove, in opposition to the theory of Sir Everard Home, that the solvent power of the gastric fluid, is not so manifest in the cardiac as in the pyloric portion of the stomach. In this age of experimental inquiry, in which we no longer *ignorantly* suppose, that when the brain is out the animal dies, it is hardly probable, that such a peculiar action of the stomach, if it really existed, should not have been witnessed. In this age of experimental investigation, it argues a degree of ignorance, and total want of zeal in the medical profession, (to both of which charges I must plead guilty,) not to have had ocular proof of, the *living* peristaltic motion of the intestines, the contraction of the *living* heart, and the digestive action of the *living* stomach. This last has been witnessed by several experimentalists, but nothing like the hour-glass contraction of the stomach has been observed. We must, I think, agree with Dr. Young in saying, "that the division, which Sir Everard Home has supposed to exist in the stomach, does not appear in itself to be highly probable; but his experiments on the two kinds of absorption, which are observable in fluids entering the stomach, and on the functions of the spleen, will lead, *if they be confirmed* by future investigations, to the most important results."* But my observations are at present confined to the effects produced by living organs, and not to the mode of action by which these effects are produced. There is nothing in the opinions detailed, which does not appear to justify the conclusion, that the food, in consequence of its admixture with the gastric fluid, and of the living action of the stomach, is separated into certain elementary particles, which are capable of re-uniting in those peculiar proportions which constitute chyle.

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* Philosoph. Transact. 1808.

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* Philosoph. Transact. 1808.

The food thus changed by the action of the stomach, consists, as before stated, of certain particles united, or capable of uniting, in those proportions which constitute chyle, and of others, which have either not been decomposed, or are not capable of re-uniting in the peculiar proportions mentioned. Dr. Hunter proved by experiment, that the food is animalized in the stomach. Some bread, which had been allowed to remain in the stomach of a dog for some hours, did not pass into the acetous fermentation, but became *putrid*. This peculiar change belongs to animal matter alone; and if that important change, by which vegetable becomes animal matter, takes place in the stomach, any secondary process appears to be unnecessary; and therefore, I conceive that the *chyle* is formed by the action of that organ.* The process which the food next undergoes, is supposed to be owing to the combined action of the biliary and pancreatic fluids; these are combined with the food immediately after its exit from the stomach; but the uses of the pancreatic fluid are unknown, and therefore, this process is usually attributed, *par excellence*, to the bile; but at the same time it is admitted, that the combined action of both, is necessary to those peculiar changes, which are found to take place in the upper portion of the alimentary canal. As in cases of jaundice, when the passage of bile into the duodenum is partially if not wholly obstructed, the body still continues to be nourished, it seems necessary to admit, that the fluid, which is prepared in the stomach, must contain in itself the principles of nutrition, and that the secondary process, which is effected by the admixture of the bile, is of minor importance. Dr. Saunders says, " an opinion very generally prevails, that the bile assists in the process of chylification, by mixing with the digested food contained in the duodenum ; for it is demonstrably proved, that the digested matter does not assume a chylous form, until it has passed below that part of the intestines, where the bilious and pancreatic juices make their entrance ;"* and yet from the experiments of Mr. Hunter we learn, that " the food was least dissolved or even mixed, toward the great end of the stomach, but became more and more so toward the pylorus ; and just within the pylorus, it was mixed with *a whitish fluid like cream*, which was also found in the duodenum ;" and in cases of jaundice, there are strong reasons for supposing, that this whitish fluid is capable of nourishing the body. "Upon the ground of these facts, (continues Dr. Saunders,) it has been presumed rather than demonstrated, that either all or some of the constituent parts of the bile contribute to chylification; it is probable however, that even admitting the bile to contribute somewhat to the digestion and assimilation of our food, its principal office is that of a natural and habitual stimulus to the intestines, keeping up their energy and peristaltic motion, which may be affected either by an increase of its quantity, or a change in its quality, produced by disease."* The nature of chyme, (the fluid admitted to be generated in the stomach,) and of chyle, (the fluid supposed to be formed in the duodenum,) has been investigated by many intelligent and scientific chemists, but more particularly by Drs. Berzelius, Bostock, and Marcet; but their experiments do not appear to point out that dissimilarity in the characters of each, which renders it necessary to infer, that the chyme must be itself *digested*, that is decomposed and re-combined, in the duodenum, before it can constitute chyle. Dr. Young says, "that the presence of the bile is not however ab-

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* Med. Literature.

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* Bread, allowed to remain in the stomach of a dog for eight hours, is so much changed, that it will not run into the vinous fermentation, but when taken out and kept in a warm place, becomes putrid.—HUNTER.

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* Saunders on the Liver.

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* Saunders on the Liver.

solutely necessary for the formation of chyle, is proved by cases of jaundice, in which the discharge of bile has been interrupted for one, two, or more weeks, and yet the person has not died, (the author might almost have said not perceptibly wasted,) for want of nourishment."* It appears then, that a fluid capable of being converted into blood is formed in the stomach, and that seems to be the only point which it is necessary to ascertain; the subsequent *digestion* of that fluid in the duodenum, we presume therefore to be unnecessary, and if unnecessary, not performed. In opposition to the opinion expressed by Dr. Saunders, that the principal use of the bile is to stimulate the intestines, Mr. Saumarez has very justly remarked, " since then the alimentary canal is constructed with a view of retarding the motion of the food along the first part of its course, it is unreasonable to suppose, that the real and direct intention of the hepatic system is destined to hasten its expulsion; if this were the case, there could not exist harmony between both; the intestines would always tend to prevent, what the bile was always destined to produce, and the ductus communis, instead of opening in the duodenum, would have opened in the rectum."*

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* Medical Literature.

Mr. Hunter says, "it appears from many experiments, that the digested or animalized part, when carried into the intestines, is attracted by the villous coat, or clings to it as if entangled amongst its villi, while the excrementitious part, such as bile, is found lying unconnected with the gut, as if separated from it;"† but Mr. Abernethy steers between these two opinions, and has, I think, pointed out the real nature of that change, which takes place in the duodenum:—"The uses of the bile have, of late, much engaged the attention of physiologists. Mr. Hunter observed, that it did not seem to incorporate with the chyle, and it certainly cannot do so, and retain its own nature, since its colour and taste are so intense, that it would impart these properties to the chyle, if mixed with it in the smallest quantity. The difficulty of conceiving that the two fluids can be agitated together by the peristaltic motion of the intestines, without becoming incorporated, has led to the opinion, that the bile may combine with the alimentary matter, and lose its original properties. Fourcroy thinks, that the alkali and saline ingredients of the bile may

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[*] Saumarez' Physiology.

[†] Observations.

unite with the chyle, and render it more fluid, while the albumen and resin may combine with the excrementitious matter. It is indeed evident, that the bile combines either totally or partially with something separated from the chyle, and exists formally in it, and in a state of health uniformly dyes it of its peculiar colour; and therefore it has of late been supposed, that the bile may serve to *purify* the chyle, by precipitating and combining with its fœculent parts."* It is probable, that the bile and pancreatic juice unite with the excrementitious and useless parts of the chyme, (that is, useless as far as the nutrition of the body is concerned,) that the chyme thus purified becomes chyle, and that the excrementitious parts acquire from this union a property, which not only favours their expulsion from the system by stimulating the intestines, but by acting as a stimulus to the absorbent vessels, causes their orifices to be closed, and thus prevents their absorption into the general system. It has been supposed, that the excrementitious parts of the chyle, in their progress through the remainder of the intestinal canal, may undergo some further changes, and the unusual size of the cœcum in the horse, has tended to corroborate this opinion. Mr. Abernethy says, "that animal will drink, at once, three or four times more water than his stomach can contain, which passes rapidly through his intestines, till it arrives at his capacious cœcum, where it lodges. He continues to eat in like manner, and the surplus of the food, must readily pass through the pylorus, before it has been completely digested; on which account he would waste a great deal of food, had not nature contrived to produce a kind of *second stomach and digestion*, in that part of the bowels called the Intestinum cœcum." I am not disposed to agree with this author in the supposition, that a *second* digestion takes place in any part of the intestinal canal. We know, that in the human stomach, undigested matter is often retained for many days; the pylorus continues in a permanently constricted state, so as to prevent its descent into the intestinal canal; it does not follow, that because water, which requires no absolute digestion, is allowed to pass rapidly into the cœcum, that therefore, undigested matter is not retained in the stomach by the discriminating power of the pylorus above-mentioned. If actual digestion occurred in any part of the alimentary canal, except the stomach, why do we observe in those animals, who are nourished by external absorption, that is, by substances conveyed immediately to that part of the alimentary tube which is *below* the stomach, that these substances are not allowed to remain there for the purpose of being digested, but are conveyed to the stomach itself?

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* Abernethy's Works.

If at this precise period of time, when our food is converted into chyle, we take a general survey of the animal frame, we find that the actions of the system are attended with a constant waste of those substances which constitute its organization, and that a supply of chyle is already provided in the intestines, for the purpose of repairing the waste incurred. The object therefore which nature has to accomplish, is to appropriate the chyle to its intended object, and to remove from the system those substances, which having lost their vitality, no longer constitute a component part of the living organization. We have already seen that the process of digestion consists, in disuniting the principles of our food, so that they may re-unite in those proportions which constitute chyle. This I consider to be the first process of digestion. But the chyle, in order that it may be converted into the component parts of the animal frame, must be conveyed to the organs of nutrition, for the purpose of being applied to its destined object, and those substances, which have already accomplished their intended functions, and have become useless and excrementitious, must be conveyed to their appropriate organs, for the purpose of being eliminated from the system. The conversion of alimentary matter into chyle, may be said to begin with those actions which convey the food to the stomach; in the same manner, the conversion of chyle into the component parts of the animal frame, and of dead animal matter into the different excretions, may be said to commence with those actions, by which the one is conveyed from the internal surface of the intestinal canal, and the other from the several parts of the animal frame: the former is the peculiar function of the lacteal, and the latter of the lymphatic absorbents. The chyle and the dead animal matter before mentioned, are conveyed through the medium of these vessels to a certain point, where they are blended together, and thus united, constitute the peculiar fluid termed blood. This action of the absorbents, is precisely analagous in object and effect, with that action of the organs of deglutition, by which alimentary matter, (consisting of certain substances capable of being appropriated to the uses of the system, and of others not endowed with this property, and therefore, as soon as convenient, eliminated from it,) is conveyed to the stomach.—It may be said, that the chyle in its passage through the absorbent system, undergoes certain changes, by which it is better calculated to form a component part of the animal frame; but this fact only renders the analogy more striking; as we have already shewn, that a similar change is produced in our food, before it arrives at the principal organ of digestion; a change, by which it is better calculated, to enter into the composition of chyle. It appears then, that the nutritious part of our food is converted into chyle, and that the excrementitious part, united with the biliary, pancreatic, and intestinal juices, is removed from the system by the peristaltic motion of the intestines. I shall proceed in my endeavour to shew, that the nutritious part of the blood is in the same manner converted into the component parts of the animal frame, by the action of its *digestive* organs, and that the excrementitious part of the blood, is in the same manner removed from the system, by the action of organs appropriated to that peculiar object. The first step in what may be denominated the secondary process of digestion—is Absorption.

CHAPTER III.

ORGANIZATION—ABSORPTION.

THERE are certain vessels, which opening on the internal surface of the alimentary canal, absorb the milky fluid denominated chyle, the origin and formation of which have been already pointed out. These vessels were denominated by Bartholin vasa serosa, but he was evidently unacquainted with the nature of their peculiar function. We have the authority of Galen for saying, that these vessels were seen by Erasistratus, but they were at that time considered to be empty blood vessels. According to Haller, they were discovered in the human subject by Tulpius; —" primus omnium vidit Tulpius."—But we are principally indebted to the labours of Asellius, Dr. Hunter, Mr. John Hunter, Dr. Monro, Sheldon, and Cruickshank, for that knowledge which we now possess of their anatomical structure and peculiar function.

The lacteals were, for a considerable time, supposed to terminate in the liver, but no doubt is now entertained, that their true course is from the surface of the intestinal canal to the thoracic duct, and that their peculiar function is to transmit the chyle from and to these given points. In consequence of the resemblance between chyle and milk, (a resemblance which is applicable only to their external appearance), the vessels conveying this fluid were denominated lacteals. The term proposed by Asellius is now almost universally adopted, but is, in my opinion, objectionable, because it conveys the idea that chyle is, what in reality it is not—milk; because the chyle of some animals bears no resemblance to milk, either internally or externally; and because it suggests the idea of a greater difference between these vessels and those of the lymphatic system, than is found to exist. Dr. Hunter says, "The lacteals arise from the surface of the intestines, pass through the conglobate glands, terminate in the thoracic duct, and are, beyond dispute, absorbents. The lymphatics resemble the lacteals in a number of circumstances; their coats are equally thin and transparent, they are equally crowded with valves, for which no good reason can be given, supposing them to be continued, as has been imagined, from arteries, for they cannot be injected from them as the red veins can; they also terminate at last in the same common trunk with the lacteals, the thoracic duct; and lastly, they too appear to rise from surfaces. Thus a grand system for absorption, in man and quadrupeds, is formed, and the lacteals and lymphatics are blended under the common name of absorbents."* Mr. Hunter, with the view of conveying in a simple and forcible manner, his ideas of the object of absorption, has exhibited, in conjunction with those anatomical preparations which are more immediately connected with this function, a common hyacinth root. "The first preparation which Mr. Hunter has put up in this department of his collection, is a common hyacinth root, which I cannot believe exhibits any thing more than may be observed with a glance of the eye, in the windows of our houses during the spring season. We see the succulent roots descending from the tuber, and the leaves beginning to bud. I cannot believe that Mr. Hunter, or others, have ever seen the minute absorbing vessels, which must, nevertheless, exist in these roots; consequently, I conclude, that he exhibited this preparation merely as an argument; as if he had said, you must grant me, that there are vessels to imbibe the juices of the earth, which afterwards become the sap or nutritive fluid of the plant; and I am convinced, there are similar vessels to perform a similar function in all parts of animal bodies."*

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* Dr. Hunter's Lectures.

There are few subjects upon which the opinions of physiologists are more divided, than that of cutaneous absorption. The question is not, whether absorption takes place from the skin, from the cutis vera, because this fact admits of daily proof; but whether substances external to the body, the air for instance, are, through the medium of those vessels which are distributed upon the skin, introduced into the system; and the question is not, whether such a phenomenon, may or may not, occasionally and under peculiar circumstances, be observed; but whether substances, exterior to the body itself, are or are not, by the constant and natural action of these vessels, conveyed into the general mass of circulation. Does the animal, for instance, feed upon the air or other substances external to the body, through the medium of these vessels? Dr. Henry says, " It is not in the lungs only, that the blood exerts an action on atmospheric air, for a similar function, it appears, belongs to the skin throughout the whole body. If the hand be confined in a portion of atmospherical air or of oxygen gas, it has been ascertained, that the oxygen disappears, and is replaced by a portion of carbonic acid ; at the same time, a considerable quantity of watery fluid transpires, and may be collected by a proper apparatus."* Is the skin, it may be asked, a respiratory organ? The arguments which have been adduced in favour of the opinion that it is so, will be considered more fully in a subsequent part of these observations. Admitting, for the present, that the air unites with the blood through the medium of the skin, does it follow that this union must be dependent upon the action of the absorbent vessels ? We have positive proof, that the air does not unite with the blood, in the process of respiration, through this medium ; and therefore, admitting the action of the skin to be precisely similar to that of the respiratory organs, the inference to be drawn from the fact, is in direct opposition to the idea, that the influence of the air is communicated to the several parts of the system through the medium of the cutaneous absorbents. It is clear, that the oxygen of the air does not unite with the blood, in the process of respiration, through the medium of these vessels, because the lymphatics pour their contents into venous blood before it arrives at the lungs; that change therefore in the appearance of the blood which we attribute to the absorption of oxygen, ought, consistently with such an idea, to take place before and not after its passage through this organ; but the fact we know to be otherwise; and therefore we presume, that the absorbents are not at all concerned in that action, by which, in the process of respiration, the oxygen of the air unites with the blood. If the cutaneous absorbents do not act upon the surrounding atmosphere, there is no reason to suppose that in a state of health, they act upon any thing which is exterior to the body itself. It may be said, that cutaneous absorption is considered to be a process, by which certain substances external to the body, unite directly and immediately with the blood, through the medium of the blood vessels: but the term cutaneous absorption, if it does not apply to the absorbent vessels of the animal frame, is clearly a misnomer, and ought to be discarded from every system of physiology. That certain substances may, by artificial means, be made to unite with the fluids of the skin, and thus become the legitimate objects of absorption, I am not disposed to deny; but such absorption is clearly morbid, and therefore unconnected with this part of my subject; and if admitted only tends to prove, that there is nothing peculiar in the action of the cutaneous absorbents.

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* Abernethy's Lectures.

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* Henry's Chemistry.

There is reason to suppose that living animal matter is never subjected to the action of the absorbent vessels. This I consider to be one of the most singular and important circumstances connected with this peculiar function. The fact, it must be admitted, cannot be demonstrated, because it is impossible to *prove*, that the matter absorbed has ever possessed, or having possessed has been deprived of a principle, the existence of which can only be inferred from the phenomena we witness, and the loss of which is not immediately indicated by any sensible change; but to the eye of reason the fact appears to be sufficiently developed. We know that dead animal matter is constantly taken up by the absorbents, and in order to promote absorption, we have recourse to those remedies which seem to produce the effect intended, by destroying the vitality of the part which is absorbed. The characteristic property of life, is to counteract and prevent, as far as is consistent with the nature of our constitutions, the disorganization of animal matter. If the absorbents are supposed to have the power of taking up any portion of living animal matter, they are supposed to be capable of acting in direct opposition to the vital principle; they are attempting to destroy what life is attempting to preserve, and we are establishing a natural warfare in the animal system. To imagine such a want of harmony in the actions of the living frame, is I conceive a libel upon the operations of nature. Dr. Barlow says, "From the investigations which have taken place respecting the functions of these vessels, it would appear, that matter deprived of its vitality, is the proper object of their operation. This may be proved by experiment; for if gradual pressure is made upon a part capable of compressing and obliterating its nutrient vessels, and thus destroying its vitality, the absorbents of the adjacent parts are immediately employed in taking up and conveying back to the circulation, the matter thus deprived of life, and a loss of substance in the part is speedily manifested; and this operation is so certain, that the principle is beneficially applied to the removal of præternatural growth, when its nature and situation admit of pressure being employed with such an intent. If the part deprived of life, is either too large to be thus removed, or too rapidly destroyed to give time for the slow operation of the absorbents, they then, by acting on the line of junction between the living and dead matter, speedily produce a separation of the latter, as is exemplified in the sloughing of ulcers, and the cure of sphacelus. We are hence, I think, warranted in concluding that the particles taken up by the absorbents lose their character of vitality before they are thus acted on; and in contemplating the quantity of dead matter, which, on this principle the living body must be continually parting with, and have at all times within it, may assign even physical accuracy to the scriptural saying, 'that in the midst of life we are in death.' "*

Nearly allied to this action of the lymphatic absorbents, is that of the lacteals, which open on the internal surface of the intestinal canal. They have been supposed to be gifted with a species of intelligence, by which they are enabled to distinguish between the chylous and excrementitious parts of our food. We have abundant proofs in pathology, that this discriminating power is not always exerted; and the observation of Mr. Hunter, that the chyle adheres to the villous coat of the intestines, and that the excrementitious part of the food remains loose in the intestinal canal, and unattached to its surface, appears to offer a sufficient explanation of the phenomenon observed. The chyle may be the only part of our food which is applied to the orifices of the absorbing vessels.

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* Med. & Phys. Journ. 1813.

I must not omit to mention, that some experiments were made by Mr. Brodie, of London, and Dr. Magendie, of Paris, to prove that absorption takes place exclusively from venous orifices, and that the vessels commonly called absorbents, have nothing to do with this process. This, it is well known, was the opinion of the earlier physiologists. Mr. Cruickshank says, " though Asellius had not seen the lacteals in men, he inferred their existence from analogy, and firmly asserted it. But this doctrine was far from being generally received; and the doctrine of Hippocrates and Galen, which taught that the chyle was absorbed from the intestines by the red veins, more generally prevailed; and Asellius's vessels were considered by the greater number of anatomists, as fictitious."* These opinions of Mr. Brodie and Dr. Magendie, have been submitted to the judgment of the medical world for nearly ten years; they have not been confirmed by observation, and are generally considered to be erroneous. The progress of science appears to be more retarded by hasty deductions drawn from a few insulated facts, than by even a blind acquiescence in the opinions of others. Facts which have received the sanction and impress of time, ought not to be questioned lightly; and it is incumbent upon every one, who advances a new doctrine, to prove that that which is generally accepted is unsatisfactory. "Much as we admire and encourage all attempts to advance physiology by experimental investigation, there is perhaps no science, where progress has been so greatly impeded by partial experiments and hasty deductions. Independently of the chance of error in these deductions, there are so many circumstances which affect the result, that we should very rarely draw positive conclusions from single experiments, however ingeniously contrived or dexterously performed. Still more must we distrust their results, when they are inconsistent with general observation, or even the doctrines generally taught in the schools, without being satisfied that we are mistaken in the former, and that the grounds upon which the latter are advanced are untenable."* The orifices of the absorbent vessels are so minute, as to have escaped observation; they have not been demonstrated; but short of actual demonstration, which the nature of the subject will not admit of, we have every rational proof, that these orifices arise from cavities and not from blood vessels. The facts, upon which this opinion is founded, I might also say established, are so well known and so generally admitted, that a repetition of them appears to be wholly unnecessary in this place; but there is one which is almost decisive upon the point in question:—" It is ascertained that Mr. Hunter injected the vessels of the medusæ from the digestive cavity, in some of the specimens preserved in the Museum, in the year 1779, and these vessels appear beautifully distributed throughout the substance of the animal, frequently communicating like the threads of net-work." These vessels could not have been thus injected, if their orifices had not communicated with the digestive cavity. It will no doubt be said, that the vessels alluded to might have been blood vessels injected from their venous orifices; but it is almost impossible to deny that the fluids contained in the digestive cavity, and those which circulate through the lacteal absorbents, are the same: if for instance the fluid contained in the digestive cavity be white, it is white also in the absorbents; if green, the fluid contained in the absorbent vessels is of the same colour. This fact cannot be explained by those who deny absorption from cavities, and imagine it to take place from blood vessels only, without supposing a double absorption (absorption from the digestive cavity, and from the blood vessels) to occur; but it will readily be admitted, that such a work of supererogation is wholly inconsistent with that simplicity of labour and grandeur of effect, which are so beautifully displayed in all the other operations of nature.

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* On the Absorbents.

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* Med. & Surg. Journ. 1814.

It has been suggested, that the lacteal and lymphatic absorbents may be distinct systems of vessels—the one opening from cavities and the other from arteries;*—but the idea is almost obliterated by time; and I think it wholly unnecessary to revive opinions which have died a natural death. The theory advanced by Dr. Hunter, that the lacteal and lymphatic vessels are the same in structure, function, mode of action, and distribution, and that they absorb from cavities only, is almost universally adopted: and there does not appear to be any good reason for questioning its accuracy.

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* Humpage on the Absorbents.

The first step in the process of stewing a carp, is, if we may believe Mrs. Glass, " to catch it;" the first step in what I have termed the secondary process of digestion, is to catch or lay hold of that substance which is to be digested. If we consider the orifices of the absorbent vessels to be so many hands distributed through the system, laying hold of every substance which has lost its vitality, and shrinking with a sensation of respect and veneration from that which is endowed with the vital principle, we shall have a very accurate idea of the nature of absorption. It is almost unnecessary to point out the analogy between this action of the absorbents, by which certain substances are taken up and conveyed to a particular part of the system, and that of the hands, by which certain substances are taken up and conveyed to the mouth. There are many animals who live by suction; in other words, by absorption; in these, the first action in both the primary and secondary processes of digestion, is literally speaking the same. The food is conveyed from the earth to the stomach, or rather to the mouth, for the purpose of being carried on to the stomach, by absorption; it is also conveyed from the digestive cavity to the thoracic duct, for the purpose of being carried on to the blood, by absorption. In some animals, the first step in the first process of digestion, is literally and physiologically speaking, absorption. "Mr. Hunter says, I have an idea that some animals absorb their nourishment similar to a spunge, and dispose of it immediately to their own increase. Professor Cuvier believes that polypes are formed of a pulpy substance, having pores that imbibe nourishment, which becomes arranged so as to constitute their bodies, and to produce their shoots or offspring."* "The hydatid seems to be nourished in like manner. Analogy would induce us to believe, that it was nourished like vegetables, by absorption from without."† In every plant the first process of digestion is wholly deficient; it has no stomach or alimentary canal, and yet its food is digested. The roots of plants absorb nourishment from the earth, in the same manner as in animals, the lacteal absorbents take up the chyle which has been already prepared in the digestive cavity; the roots of plants select from the earth those principles which are capable of being converted into a component part of their organization, and reject the others. This fact may be inferred from the circumstance, that the ground is exhausted in different ways by different plants, and is more readily restored by one kind of manure than another; it may also be inferred from the circumstance, that some plants will not grow in the earth at all, the earth, it may be presumed, not containing its natural food. "The *Cuscuta Europea* is a parasitical plant. The seed opens and puts forth a spiral body, which does not seek the earth to take root, but climbs in a spiral direction, from right to left, up other plants, from which, by means of vessels, it draws its nourishment. A better-known parasitical plant is the evergreen shrub called the *mistletoe.* What we have to remark in it is a singular instance of *compensation.* No art hath yet made these plants take root in the earth. Here therefore might seem to be a mortal defect in the constitution. Let us examine how this defect is made up to them. The seeds are endowed with an adhesive quality so tenacious, that if they be rubbed upon the smooth bark of almost any tree, they will stick to it; and then what follows? roots springing from their seeds insinuate their fibres into the woody substance of the tree; and the event is, that a mistletoe plant is produced next year."* The analogy between this discriminating and selecting power of roots, and the action of the lacteal absorbents in animals as already pointed out, will readily be admitted.

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* Abernethy's Lectures. † Ibidem.

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* Paley's Natural Theology.

These roots convey also the absorbed substances to vessels which are distributed through every part of the system, in the same manner, as the lacteal absorbents in animals convey their contents to the blood vessels. Absorption is the first link in the chain of actions, which may be said to constitute the digestive function of plants, and in animals, generally speaking, the first process in what I have termed their secondary digestion. Professor Cuvier says, "With respect to nutrition, vegetables which are attached to the soil, absorb immediately, by their roots, all the nutritive parts of the fluids which they imbibe. These roots are subdivided to extreme minuteness: they penetrate into the smallest interstices, and proceed, if it may be so said, to seek at a distance food for the plant to which they belong. Animals on the contrary, which are not fixed, and which frequently change their place, can *transport with themselves* a portion of the substances necessary for their nutrition; they have therefore received an internal cavity, in which they deposit the matters destined for their aliment;

and the inward surfaces of this cavity are furnished with innumerable absorbing pores or vessels, which according to the energetic expression of Boerhaave, *are real internal roots*."*

The absorbents, in their passage from their commencement in cavities to their termination in the thoracic duct, are universally intercepted by certain organs, which from their supposed resemblance to nuts are not unfrequently termed kernels, but more generally glands. The glands which constitute a part of the absorbent system, are by way of distinction, and in consequence of their usual and peculiar form, denominated *conglobate;* while other organs which have also been denominated glands, and which are unconnected with the absorbent system, are termed *conglomerate.* Mr. Hewson supposed that there were occasional exceptions to the general rule, that absorbents are in their course intercepted by glands; but Mr. Cruickshank says, " we may inject vessels from the thumb which shall run along the whole extremity without entering any gland till they come to the clavicle; but it *never* happens, that they terminate in the thoracic duct or the right trunk of the lymphatics, without having first passed through glands."†

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* Comparative Anatomy.

† Cruickshank on the Absorbents.

Great difference of opinion is entertained as to the minute anatomical structure of these organs, some contending that they are simply convolutions of the lymphatic vessels, while others as strenuously maintain the opinion, that they are a congeries of cells totally distinct from the lymphatic system. The anatomical structure of a part, does not of itself give us much insight into the nature of its function. What anatomist, for instance, by examining the structure of the liver, the stomach, or the parotid gland, could *a priori* say, that one is calculated to secrete bile, another the gastric fluid, and another the saliva: we can have no idea of the function of an organ, but by comparing the phenomena we witness, that is the effects produced, with the circumstances which have preceded them. It would therefore be altogether inconsistent with the object of this work, to enter into any minute anatomical disquisition as to the structure of an organ. The opinion generally entertained upon this subject, may be collected from the following anatomical description, and I am not aware of any circumstances which should lead us to consider it as inaccurate. "The cellular texture of glands does not appear to be any longer questioned; they are described by some anatomists as being composed of cells internally, while others consider them as being a congeries of convoluted absorbent vessels: most of the glands have much of the former, and many of them of the latter appearance. The lymphatics, when they approach or come in contact with the gland, split into radiated branches, which after spreading out upon it, penetrate into its substance; from the opposite side of the glands, vessels go out in the manner they entered them, and are termed *vasa efferentia*; they are frequently, but by no means always, fewer in number but larger than the *vasa inferentia*."* A gland, according to this description, seems to bear a very close resemblance to a spunge; and the effect of such a structure, is evidently to retard the passage of the absorbed fluids. The extent and universality of the glandular system in the human body, would lead us to suppose, that the absorbed fluid is thus retarded for some very important purpose; but on the other hand, the total want of a glandular system, (confining this term to the absorbents,) in whole classes of animals, is in direct opposition to such a conclusion. Professor Cuvier says, " vertebral animals differ among themselves with regard to the colour of the chyle, which is white and opaque in the mammalia, and transparent like the other lymph in birds, reptiles, and fishes. The three last classes therefore, have *no conglobate glands* in their chyliferous vessels, while they are very numerous in the first."* In these animals then, chyle is converted into blood without the intervention of a glandular system; we may therefore presume, that in the human frame, the function of the conglobate glands is not so important, as from their extent and universality might at first be imagined. We cannot suppose that glands consist of convoluted absorbents, so arranged, for the *sole* purpose of retarding the progress of the chyle, because no apparent advantage is derived from the circumstance. There are many examples of a peculiar distribution of the blood vessels, which is evidently calculated to retard the passage of the blood. The blood, if not thus interrupted, would often injure, if not destroy the delicate structure of those organs to which it is directed; we have no hesitation therefore in admitting, that such a peculiarity of distribution has been provided for that *sole* purpose, and we do not look for any further explanation of the phenomenon. Dr. Monro says, "The rete mirabile Galeni, situated on each side of the sella turcica, about which there has been so much dispute, is very remarkable in most quadrupeds. Galen seems with justice to suppose, that this plexus of vessels serves for checking the impetuosity of the blood destined for the brain."* But no such evident advantage is derived from this peculiarity of structure in the glands; we presume therefore that delay is not the *sole* object intended.

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* Fife's Anatomy,

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* Comparative Anatomy.

Is the chyle poured out into the cellular texture of the glands for the purpose of being re-digested? Such a supposition is highly improbable, because the action itself is apparently unnecessary. We know that the chyle, as prepared by the stomach, is already animalized, and there is great reason to believe, that in its nature and composition it approximates very nearly to blood. "The experiments of Hallè, Emmerst, and Reuss, agree in shewing that chyle in every thing but its colour resembles blood, but is much more diluted."† I am not aware that chemists have discovered any material difference in chyle, as it exists before and after its passage through the glandular system; but comparative physiology seems to throw a gleam of light upon this interesting point. The chyle in birds, reptiles, and fishes, who have no conglobate glands, is stated by Cuvier to differ from that of the mammalia in being, not white and opaque, but transparent. Dr. Young says, "chyle from its colour and its coagulating when boiled, was for a long time compared to milk, and supposed to contain a large portion of sugar of milk; but this erroneous hypothesis is now corrected. In the thoracic duct the milky appearance is less conspicuous, since it is diluted by the lymph of the lymphatic absorbents, and it is commonly of a yellow grey colour."* It is admitted then that chyle, after it has passed through the conglobate glands, is more transparent than it was before; the "milky appearance is less conspicuous;" "it is commonly of a yellow grey colour." Chyle then, after it has been subjected to the action of the glands, approaches nearer in its appearance to that of birds, reptiles, and fishes, in which no glandular system is required. As digestion in the glands is clearly unnecessary, we presume that their only action, is to separate from the chyle those substances which are adventitious, and do not properly belong to it; and that in consequence of this separation, the chyle becomes more transparent, and more similar to that of birds, reptiles, and fishes, in which this purifying process is wholly unnecessary. To what cause shall we attribute the circumstance, that the chyle of the mammalia should be more impure than that of birds, reptiles, and fishes? In birds, the great object of nature appears to be that of rendering their bodies as light and buoyant as circumstances will admit of. A considerable quantity of food contained in the stomach would be manifestly inconsistent with this object, and therefore birds feed often and digest rapidly.* There is strong reason to suppose, that the food of birds is more plain, simple, and easy of digestion than that of other animals; and that the natural food of each bird is almost confined to one particular substance—to that which it is capable of digesting easily and rapidly. We know from experiment and observation, that young pheasants and nightingales will not thrive, unless provided with that peculiar and *individual* food which is natural to each. It seems then that the food of birds is less varied, and consequently more easy of digestion, than that of most other animals. We presume that digestion is not only more rapidly, but more perfectly accomplished in the stomach and intestinal canal of birds, than in those of other animals; because if the chyle were detained in its passage through the absorbent system by the interruption of glands, for the purpose of being purified, the object of nature, which is clearly that of diminishing the specific gravity of birds, would be thus counteracted. Reptiles on the contrary feed but seldom, and digest very slowly. The actions of the system, are generally speaking, languid and sluggish, and therefore attended with less expenditure of organization; that is, the particles of which their bodies are composed, do not lose their vitality and become the objects of lymphatic absorption, so rapidly as those of other animals, whose actions are more constant and energetic.

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* Munro's Works.

† Young's Med. Literature.

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* Young's Med. Lit.

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* Professor Cuvier says, "This is the reason why birds have in general an exceedingly strong stomach, and a very quick digestion."

A frequent supply of food therefore is not so necessary to reptiles, as to birds and the mammalia in general. We know that digestion is accomplished in the stomachs of reptiles more tardily than in those of the mammalia; but we presume that it is at the same time more perfect, because the necessities of the system being less urgent, no good object could be answered, by allowing the food to pass into the system in an imperfectly digested state. We see many advantages which accrue to fishes, in consequence of their specific gravities being diminished as far as circumstances will admit of; we see also that nature has provided, (in the swimming-bag,) an apparatus for that peculiar purpose.

The analogy between fishes and birds, is in this respect very remarkable; but not more so, than in that want of conglobate glands, which we observe in each. The same arguments which were adduced in favour of the opinion, that digestion is more rapidly and perfectly accomplished in the stomach and intestinal canal of birds, than in those of the mammalia, are equally applicable to this function in fishes. But in the mammalia the whole system appears to be reversed: their food is less easy of digestion, either in consequence of its variety or of its nature; and the demands of the system are at the same time so constant and uninterrupted, that the slow digestion of reptiles would be clearly inadequate to their support. Their food therefore, being from the circumstances mentioned less easy of digestion, cannot consistently with the demands of the system, remain in the instestinal canal until digestion is perfectly accomplished. How has nature guarded against the inconveniences which might be expected to arise from this circumstance? The food passes through certain organs denominated glands, which may be considered as so many strainers, separating from the chyle, which has been thus imperfectly formed, those substances which are not calculated to unite with the blood, and which, if admitted into the circulation, would lead to much constitutional distress and inconvenience. It will readily be granted, that the slow digestion of reptiles would be inadequate to the demand of the human system ; but why, it may be asked, should the stomach of the mammalia be incapable of digesting perfectly the food which is presented to it ? This question, as applicable to man, may be answered by the assistance of another: why do we constantly eat, what we know full well our stomach is incapable of digesting ? We do not even exert that discriminating power which has been given to us ; we do not select our food by instinct, as animals do, but indulge in all the variety which imagination can suggest, or a depraved appetite desire. Man has been defined to be an omnivorous animal ; and I think it will be found to be a fact applicable to the mammalia in general, that they are more exposed to circumstances which interfere with easy and perfect digestion, than those animals which have no chyliferous glands. This subject may be further elucidated by a few observations upon the *diseases* of the glandular system. When we consider the distress occasioned by the introduction of foreign matter into the blood, a fact which is satisfactorily proved by the experiments of transfusion, it is reasonable to suppose that nature has guarded as much as possible against this inconvenience. We know that the lymphatic absorbents, at different times and under different circumstances, are under the necessity of conveying to the blood a variety of substances which are useless and even pernicious to the system; we know also, that certain medicines may be introduced into the course of circulation, through the medium of the digestive organs and the lacteal absorbents. These vessels cannot, I think, be supposed to possess that discriminating power which has been attributed to them; for it must be admitted, that the medicines thus introduced into the system, are not *always* adapted to the complaint which they are intended to relieve; and we know, that some of the most serious diseases to which the constitution is liable, are wholly dependent upon this action of the absorbents. Mr. Cruickshank says, "the lymphatics and lacteals take up irritating substances not generated in the system: the infectious matter of disease from other persons, poisons, animal vegetable and mineral, from different quarters."* I cannot agree with this author in attributing absorption to a provision of nature for the artificial cure of diseases. He says, "in the fifth place, the lacteals and lymphatics introduce medicines into the blood vessels, and by this means prevents as well as cures diseases."† We find that certain poisons, and perhaps we might say medicines, will not destroy life, except they are taken up by the absorbents and introduced into the course of circulation. How can we consider that to be a provision of nature, which is apparently productive of more injury than advantage to the animal frame? The absorption of extraneous matter, must, under all circumstances, be considered a morbid action; and there is reason to suppose, that all extraneous matters are, as far as the powers of nature can accomplish, deprived of their useless and pernicious properties in their passage through the absorbent glandular system. In small pox for instance, the absorbents evidently take up the variolus matter; but this matter must have undergone some material change in its passage to the blood, because this fluid is incapable of communicating the disease to another individual. An inquiry into the nature of infection, might place me under the necessity of admitting, that the blood of a variolus patient, must contain a portion of that matter in its active state, although it is not capable of being demonstrated; I shall therefore confine the observation to siphylis and psora. The blood of persons labouring under these two infectious diseases, will not communicate either to another individual; but we know that the matter of each, *previous* to its introduction into the absorbent system, is highly contagious in its nature. An important change therefore must have been effected somewhere, and this change appears to have taken place previous to its admission into the blood. There is no intervening organ between the extremity of an absorbent and its termination in the blood vessels, the gland excepted, to which this change can well be attributed.

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* On the Absorbents. † Ibidem.

There are other pathological facts, which seem still further to countenance the idea, that the peculiar and appropriate function of the conglobate glands, is to purify and prepare for the purposes of the animal economy, the fluids which are subjected to their action. In proportion as these fluids are diseased, in other words, in proportion as this purifying process is essential to the welfare of the system, we discover upon dissection, changes more or less distinctly marked in the structure of the glandular system. Dr. Hunter has given the following account of the appearances observed in the body of dirt eaters after death. " In the colon there are frequently found large concretions of the earthy matter which they have swallowed ; this often lines the cavity of the gut, and almost completely obstructs its passage ; the mesenteric *glands* are *always* diseased."* It is well known that Dr. Stark fell a victim to his zeal in endeavouring to ascertain, experimentally and upon his own person, the peculiar effects of different kinds of food. His body was examined after death by Mr. Hunter and Mr. Hewson. " Upon opening the abdomen two or three ounces of water were discovered in the pelvis ; the bladder contained about six ounces of urine of a natural colour ; the small intestines appeared very red and inflamed at different parts ; and upon opening them, this was found to be an affection of the glandulæ peyeri ; one cluster of them seemed ulcerated ; some of the glandulæ solitariæ were of the size of a split pea ; the *mesenteric glands* were likewise enlarged, and when cut were very soft and tender."* The tendency of certain kinds of food to produce diseases of the absorbent system, has been established by the experiments of Dr. Jenner. Upon the presumption therefore that diseased organization is the consequence of diseased action, I presume that in those cases, the function of the glandular system is præternaturally increased, and that it corresponds with the nature of the fluid, and of those changes which are necessary to its healthy assimilation with the blood. If the action of the glandular system is found to correspond with the nature of the fluid, and with the necessity of certain changes in its constitution, we have reason to conclude, that the peculiar function of the glands, is to produce those changes which we witness, or rather infer to have taken place.

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* Dunc. Med. Comment.

The action of the glandular system appears to be similar, in effect, to that by which certain parts of our food, which impart to the organs of taste a disagreeable sensation, are rejected from the system, as not calculated to enter into the composition of chyle and animal matter, and therefore not allowed to come into contact with the organs of digestion. The analogy, between the roots of a hyacinth and the absorbents of the animal frame, has already been alluded to; but if we extend our views of the absorbent system, and include the conglobate glands, a potato will present us with a more apposite and correct illustration of their structure and function. The potato is the gland of the plant; and I think it probable, were we sufficiently acquainted with the nature of vegetable digestion, that the function of this organ would be found to correspond with that of the chyliferous glands in the animal frame. It is not improbable, that Mr. Hunter might have extended his views further than have been explained by Mr. Abernethy, and that the bulb of the hyacinth might have been intended to represent the gland, and the roots the absorbents of the plant. Many of our most active medicines are prepared from the bulbs and tuberous roots of the vegetable kingdom. May we from this circumstance infer, that the acrid matter contained in them requires to be purified or changed, before it is allowed to circulate through the body of the plant? The analogy between this action of the glandular system, and that by which the nauseous part of our food is rejected, is sufficient to justify us in considering glandular action, as only a *preparatory* step to the secondary process of digestion.

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* Dunc. Med. Comment.

The fluids having passed through the lacteal and lymphatic glands, are carried on by a continuation of the absorbent vessels, termed vasa efferentia, to the thoracic duct. It does not appear that any change is produced by the action of this organ. It is merely to be considered as a trunk of communication between the absorbents and the blood vessels; it is in reality one large absorbent, terminating in the angle formed by the union of the left internal and jugular veins."*

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* There is nothing peculiar in the smaller thoracic duct, considered *physiologically*.

CHAPTER IV.

ORGANIZATION—ASSIMILATION.

WE have thus traced our food from its origin to its union with the blood. Many authors have imagined that the chyle is not perfectly assimilated with that fluid, until it has been subjected to the action of the respiratory organs. Now the blood can be traced to no other sources, the air excepted, than the fluids mentioned; and therefore we must admit, that these fluids contain in themselves, at least, the principles from which the blood is formed. We are told that "the experiments of Hallè, Emmerst, and Reuss, agree in shewing, that the chyle, in every thing but its colour, resembles blood."* We must therefore admit it to be probable, that chyle contains not only the principles but the ingredients of that fluid. The simple union of these ingredients with the corresponding ingredients of the blood, may constitute that process which has been termed Assimilation. There is not that difference, the colour excepted, between the blood itself and the fluids conveyed by the several absorbents of the system, which renders it necessary to suppose, that any thing further than a simple union of the various ingredients contained in each, takes place upon their being brought into contact.

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* Young's Med. Literat.

The only apparent change which the absorbed fluids undergo upon their admixture with the blood, is a change in their colour from white to red. This circumstance, considered by itself, does not necessarily imply any thing further than a combination of the several ingredients contained in each; but there must be a source from whence this colour is derived. "The new matter of the body is supplied from the intestines, and the chyle in the absorbents of the mesentery, (might not the author have said in the thoracic duct?) is white: whence then does it derive its change of colour? It is said to be produced by its mixture with the blood; but the blood is totally renewed within a definite time; and if the alteration in the colour of the chyle is assignable to this cause, in proportion as the blood is wasted and renewed, it ought to become progressively pale, until it had attained to the whiteness of the chyle itself. We must look then for some *source* of colorification;

and where will it be found?"* It is evident that this property of chyle must be acquired in the course of its circulation through the blood vessels; that is after and not before its admixture with the blood. We have the strongest reasons for supposing, that the blood acquires an additional ingredient in its passage through the lungs, and we might naturally look to these organs, as the sources of that remarkable change which takes place in the colour of the chyle; but any consideration of this subject, in the present stage of my inquiry, would evidently be premature.

It has been supposed that the absorbed fluids are not perfectly assimilated with the blood until they have circulated through the lungs, that is until they have been exposed to the influence of the air. I am not acquainted with any facts, which render it necessary to admit, that the influence of the air acts as a bond of union between the particles which are to be assimilated. There is nothing in the appearance of venous blood, immediately before its entrance into the lungs and after its exit from these organs, which justifies the opinion, that the process of assimilation has been accomplished in the intermediate period: and even admitting this to be the fact, the phenomenon witnessed, may as fairly be attributed to the action of the blood vessels themselves as to the influence of the air. It is more probable, that the union of the absorbed fluids with the corresponding ingredients of the blood, is not accomplished by the action of one particular organ, but that it gradually takes place in the course of its circulation through the blood vessels; and the fact, that the blood upon being drawn from the system, often presents a white, shall I say a chylous appearance, without an apparent deficiency in the action of any organ to which the phenomenon can be assigned, appears to render the opinion more than probable. There is no evidence that the lungs are at all concerned in that series of actions by which, (to use the quaint expression of Dr. Paley,) "what enters at the *mouth* finds its way to the fingers' ends." The peculiar and appropriate function of these organs, is to produce that change in the air we breathe, which may render that element available to the purposes of the animal economy. We observe a very intimate connection between the supply of air and the continuance of living action; the lungs have enough to do in feeding the functions of the system; I am therefore very unwilling to burthen them with an additional task which appears to be wholly unnecessary. Assimilation then may be considered, to be a simple union of the several ingredients contained in the absorbed fluids, with the corresponding ingredients of the blood.

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* Pring on the Absorbents.

CHAPTER V.

Organization—Respiration.

Digestion, as before stated, consists in disuniting the principles of our food, that is, of the digestible and nutritious parts of our food, so that they may re-unite in those peculiar proportions which constitute chyle, and in separating from the mass those parts which are useless and excrementitious, in order that they may be, as soon as convenient, eliminated from the system. "The function of Respiration consists of two distinct actions:—that of *inspiration*, by which the air is drawn into the lungs, and that of *expiration*, by which it is expelled, after having served the purpose for which it was inhaled." "By an easy natural inspiration, about twenty cubic inches may, perhaps, on an average, be the quantity taken in. It appears also, from the recent experiments of Messrs. Allen and Pepys, that the same quantity is expired with little if any diminution. Atmospheric air, after being once only admitted into the lungs, returns charged with 8 or 8½ per cent of carbonic acid gas. If the same portion be breathed repeatedly, considerable uneasiness is experienced ; but the quantity of carbonic acid cannot be increased beyond 10 per cent. When the state of the expired air is examined by eudiometrical tests, a quantity of oxygen is found to have disappeared, equal in volume, according to the experiments of the same accurate chemists, to the carbonic acid which has been formed. The only change then which has been satisfactorily proved to take place in respired atmospherical air, is the removal of a certain quantity of oxygen, (its nitrogen being wholly untouched,) and the substitution of a precisely equal volume of carbonic acid gas."* The process of digestion is here satisfactorily described. The principles of the air are *disunited*; the oxygen, that is the nutritious part of the air, is lost in inspiration, and nitrogen, its excrementitious part, is separated from it, and eliminated from the system by expiration. I shall have occasion, in a future part of these observations, to advert to the generally received opinion, that the oxygen lost is contained in the carbonic acid expired ; but I cannot enter upon this part of my subject at present, because it is intimately connected with the opinions I entertain of living action in general, which opinions have not yet been presented to the consideration of my reader. I shall therefore, for the present, assume the fact, that the oxygen contained in the carbonic acid expired, is *not* the individual oxygen which was conveyed to the lungs in the previous inspiration, that is, the inspiration *immediately* antecedent to its evolution from the system in the act of expiration. That the oxygen of the air unites with the blood in its passage through the lungs, appears to be admitted by the concurrent testimony of the most eminent physiologists. Mr. Saumarez says, " That the action of the lungs upon the air, by means of which it becomes assimilated to the nature of the animal system, and that it is the *union* of this vital air with the blood, which constitutes the means by which it becomes fitted to be conveyed to the different parts of the system, is proved by the negative of the case, either when the air is altogether excluded, or when air is introduced, that does not possess the aptitude to be acted upon by the lungs."* Dr. Paris says, "it was reserved for Bichât to offer a true explanation of this phenomenon ; he has very justly stated that, in consequence of the suspension of the respiratory organs, the coronary vessels, by which the muscular structure of the heart is supplied, are compelled to carry black instead of florid blood ; a fact, which in itself is quite adequate to explain the cause of the heart ceasing to contract; for the irritability of this, like that of every other muscle, can be alone maintained by *duly oxygenized* blood."* Professor Cuvier says, "In the animals that have a circulation, the blood appears to be merely a vehicle which is continually receiving from the alimentary canal, from the external surface of the body, and from the lungs, different substances which are intimately incorporated with it."† As the oxygen of the air is the only principle lost, we must conclude that oxygen is the substance which the "blood derives from the lungs," and which is incorporated with it. If the oxygen of the air unites with the blood, and circulates through the system, it cannot possibly exist in the carbonic acid expired ; and the difference between arterial, that is duly oxygenized, and venous, that is imperfectly oxygenized blood, from whatever part of the system it is

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* Henry's Chemistry.

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* Physiology.

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[*] Med. Jurisprudence.

[†] Comparative Anatomy.

drawn, is a satisfactory proof, that it does circulate through the system. Oxygen is a principle or substance which is characterized by very peculiar properties, and these properties are readily cognizable by chemical tests: and yet, notwithstanding the laborious and frequently repeated analyses of arterial blood, that is, of blood which has recently derived oxygen from the air in its passage through the lungs, we are unable, by the most delicate tests, to shew, that arterial contains an *iota* more of oxygen than venous blood. Dr. Henry says, "We are ignorant of those precise differences which constitute the distinctions between venous and arterial blood, or in what way the function of respiration converts the former into the latter."* Had it been proved that arterial contained more oxygen than venous blood, no difference of opinion could now exist as to the fact, whether the oxygen of the air does or does not unite with the blood in its passage through the respiratory organs. We are therefore under the necessity of admitting that, according to the views of the most eminent physiologists, with the exception of a few individuals, the oxygen of the air does unite with the blood, but cannot subsequently be discovered in it. How can we reconcile this fact with the phenomenon observed, without allowing that the oxygen must have entered into a new combination, by which its original properties are lost. This product of respiration, without at present adverting to the manner in which it has been formed, may be and has been denominated the *arterial influence.* In chyle, the product of digestion, we cannot discover the principles which previously existed in our food; in the arterial influence, the product of respiration, we cannot discover the principles which pre-existed in the air. It appears then, that the component parts of the air are first disunited; that oxygen, the nutritious part, combines with some other substance in those peculiar proportions which constitute the arterial influence; that nitrogen, the excrementitious part, is separated from the air for the purpose of being eliminated from the system by the act of expiration; and that the product of these combined actions, is a substance essentially different from the atmosphere to which its origin must be attributed. The analogy, between the effects produced by respiration upon the air we breathe, and by digestion upon our food, is strikingly apparent. It may be said that the analogy is destroyed by the admission, that the arterial influence is a compound of oxygen and of some *other* substance which is not contained in the air; but it remains to be proved, that chyle may not also be a compound of certain principles contained in our food, and of some other substance, which is not derived from that source. I am of opinion, that living action is always accompanied by the evolution of a certain substance, which constitutes a part of the living organization. If it shall appear, that this substance unites with the nutritious part of our food in the process of digestion, and also with the oxygen of the air in the process of respiration, constituting in the former instance the chyle, and in the latter the arterial influence, the analogy will be still further preserved. I shall, for the present, assume the fact that it is so, and upon the ground of the analogy already pointed out, venture to say, that the air is *digested.* Mr. Saumarez says, " On the contrary, it appears to be more reasonable to suppose, that the separation of different parts of the air, which takes place, proceeds from the power of the lungs alone, acting upon and separating those parts of the air which possess the aptitude to be acted upon by them; that it is a living and not a chemical act, similar to the act of *digestion* by the stomach."* I shall conclude this part of my subject by stating, that respiration, as far as we can judge from the effect produced, is a digestive process, and that the air, in its passage through the lungs, is in reality *digested.* The secondary effects of respiration are the support and regulation of living action; these are much more apparent to the senses, than the process of digestion which has been described, and will be considered more fully in a subsequent part of this inquiry.

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* Henry's Chemistry.

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* Physiology.

CHAPTER VI.

ORGANIZATION—CIRCULATION.

THE peculiar function of the Circulating System, (confining that term to the *heart* and *arteries*, which impel the blood from the heart as a given point to every part of the animal frame, and to the *veins*, which bring it back to the same point from whence it was impelled,) is too well known, and too generally admitted to require many observations. I am now speaking of the *effects* produced by living action; and the acknowledged effect resulting from the action of the heart arteries and veins, is to convey the blood from a given point to every part of the system, and to bring it back again to the same point, thus forming a kind of circle, from whence the name has been derived. The object of this circulation is, that the nutritious portion of the blood may be converted into a component part of the animal frame, and that its excrementitious portion may be eliminated from the system. There are many peculiarities to be observed in the mode of this distribution; but these peculiarities are merely to be considered as different arrangements, by which the object may be accomplished in a manner which is consistent with the peculiarities of each individual. The object is still the same; I shall therefore defer the consideration of this subject, until I inquire into the nature of that action by which the effect is produced, or into the ultimate object of every living action, viz. the preservation of the existing union between life and matter; and take this opportunity of inquiring into the nature of that fluid, which is thus distributed to every part of the system.

The blood is a fluid *sui generis*, that is, different from every other with which we are acquainted, consisting of certain substances derived through the medium of the absorbent system from every part of the animal frame, and through the medium of the respiratory organs, from the surrounding atmosphere. All attempts to ascertain the nature and constitution of this fluid have hitherto proved fruitless. We are absolutely unacquainted with the nature and peculiar properties of those substances from whence it is immediately formed: we know, for instance, that it consists of a certain fluid derived from the intestinal canal through the medium of the lacteal absorbents, of another fluid derived from every part of the body through the medium of the lymphatic system, and of a certain substance derived, through the medium of the respiratory organs, from the surrounding atmosphere. We know that the first, denominated chyle, and that the last, denominated oxygen, are destined to be conveyed to the several parts of the system, for the purpose of being incorporated with it, and we have the strongest reasons for believing, that the other substance, denominated lymph, having already accomplished its destined function in the system, is intended to be conveyed to different organs of the body for the purpose of being eliminated from it; but we are absolutely unacquainted with the nature of chyle, oxygen, or lymph. This observation, as applied to oxygen, may at first sight appear untenable; but we have every reason to suppose that oxygen does not unite with the blood, so as to form a component part of that heterogeneous fluid, until it has previously united with some other substance, by which its characteristic properties are lost. We have ascertained by chemical experiments, that blood, when no longer subjected to the living actions of the system, may be decomposed into certain substances, but we cannot, by any re-union of these substances, produce a fluid which bears the least resemblance to blood, as it exists either in its living state, or when removed from the animal frame. We have ascertained that chyle, when removed from the body, may be decomposed into certain substances, but we cannot, by any re-union of these substances, produce a fluid which bears the least resemblance to chyle, as it exists in its living state, or even when removed from the animal frame. We may ascertain by chemical experiments, what are the component parts of a seed, a leaf, or a stalk; at least we may imagine ourselves to have ascertained them; but we cannot, by any re-union of those parts, produce a whole which bears the least resemblance to either of those substances; the fact is, we cannot imitate the living actions of the system. We admit that the peculiar property of life, is that of counteracting all the known chemical affinities, and at the same time contradict ourselves, by endeavouring to point out a resemblance between, or by assuming the identity of, the laws of chemical and living action. There is not one fluid of the body, however simple, which is capable of being formed without the intervention of a living organ, and the action of this organ, we have no means of imitating.

Without detracting from the merits of those ingenious and celebrated writers who have directed their attention to the subject of animal chemistry, I may be allowed to observe that they do not *always* seem to remember, that the living organ is a tool which they cannot manufacture, and which, if formed to their hands, they would most probably be unable to work with. "Gentlemen," said Mr. Hunter in his lectures, "physiologists will have it that the stomach is a mill—others that it is a fermenting vat—others again that it is a stew-pan—but in my view of the matter, it is neither a mill, a fermenting vat, nor a stew-pan, but a STOMACH, Gentlemen, a STOMACH!"* I acknowledge myself to be under the greatest possible obligation to those numerous experimentalists, who have scattered the fruits of their labours amongst the medical profession; I have tasted some, and gorged upon others; but I have attempted to adhere to that rule of diet which was proposed by Dr. Paley:—"True fortitude of understanding consists in not suffering what we know to be disturbed by what we do not know. If we perceive an useful end, and means adapted to that end, we perceive enough for our conclusions. If these things are clear, no matter what is obscure—the argument is finished."* Instead therefore of saying, that the blood is composed of a watery fluid, soda in an uncombined state, muriat and phosphat of soda, phosphat of lime, iron, and other chemical substances, (a fact, which if applied to blood as it exists in the living body, admits of being questioned,) I would rather say, that the blood is a fluid *sui generis*, with the nature of which I am unacquainted—that it consists of the fluids derived through the medium of the absorbent system from every part of the animal frame, and of a certain substance derived from the atmosphere; and I consider it to consist of these substances, because I am unable to trace its origin to any other sources. My reason tells me, that that fluid, the chyle, in the formation of which so many different processes are employed, must be intended to answer some important purpose, and that that fluid, the lymph, which has already accomplished its destined function in the system, must be intended to be removed from it;

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* Paris' Pharmacologia.

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* Natural Theology.

and I conclude this to be the fact, because I find that a constant supply of those substances which ultimately become lymph, is conveyed into the system, and this implies that a constant expenditure is going on. We cannot suppose that, in a state of health, any part of the system is intended to be rejected from it, before it has answered the peculiar purpose for which it was intended, and therefore I presume, that no substance becomes the legitimate object of lymphatic absorption, before these purposes have been accomplished. I "perceive the useful end, and the means adapted to that end, and therefore perceive enough for my conclusion." I appear to have a sufficiently clear idea of the nature of the blood, by considering it in these two points of view, and therefore am unwilling to suffer "what I (seem to) know, to be disturbed by what I do not know." I by no means wish to discourage the *rational* application of chemistry to the phenomena of the living system, and I think it probable, that the lately discovered property of the galvanic influence, (I allude to its power of counteracting all the known chemical affinities,) may ultimately throw much light upon the nature of those phenomena, which are found to be the consequences of living action; but who can withhold a smile, upon being told in sober serious earnest, that every motion of the body is produced by an explosion of oxygen and hydrogen!

We have ascertained, that *out* of the body, the blood undergoes certain changes; it separates into two distinct portions, which have been denominated serum and crassamentum, and these portions may be still further decomposed into albumen, gelatine, fibrine, and red particles; but is it certain, that those substances which are discovered in blood after its removal from the system, did really exist in it, in the same proportions and the same combinations of their elementary principles, before its removal? It is probable, and therefore I do not object to the cautious and rational application of this observation to the state of the blood as it exists in the animal frame; but are observations of this nature always applied with that caution, which the acknowledged difference between the laws which regulate the actions of animate and inanimate bodies, seems to require? The blood for instance, when removed from the body, is found to coagulate. We are unable to account for this circumstance chemically, and therefore we attribute it to a living action of the blood itself, to a *living* action out of the body, which does not occur in the body itself. Mr. John Hunter, to whom as an anatomist and a physiologist we are more indebted than any other writer with whom I am acquainted, and whose opinions therefore must always be received with respect, if not implicitly adopted, has considered the coagulation of the blood, which is observed to take place soon after the removal of that fluid from the body, to be the effect of a living principle residing in the blood itself; and yet it is clear, that such a change taking place in the body, in the course of circulation for instance, would be attended with the general death of the system. How can we consider that to be a living action, which never occurs in the living system, except as the consequence of disease ?

There is no subject upon which the opinions of physiologists are so much at variance, as that of the vitality of the blood. I shall have occasion to consider this subject more particularly, when I advert to the different theories which have been advanced by physiologists, with the view of explaining the nature and operations of the living principle. It is sufficient for my present purpose to say, that without fluidity, the blood must be looked upon as a useless mass, deprived of that property by which alone, in its healthy state, it is capable of being converted to the purposes of the animal economy. Mr. Hunter supposes, that the fluidity of the blood is dependent upon the action of the containing vessels. He is of opinion, "that the fluid state of the blood is connected with the living vessels, which is its natural situation, and with motion; and that when there is a full power of life, the vessels are *capable* of keeping the blood in a fluid state."* So that the property of life is given to the blood for the purpose of producing that very change in its condition, which would be inconsistent with the continuance of life; and nature is supposed to have endowed the living vessels with the power of performing a certain action, the sole object of which, is to counteract that very property, which Mr. Hunter, in his imagination, has given to the blood.

Much as I respect and venerate the abilities, the laborious exertions, and the original ideas of this celebrated and justly eminent physiologist, I cannot surrender my judgment to an hypothesis, which appears to be so contradictory in itself, and so inconsistent with that harmony of design and effect, which characterizes all the operations of nature; neither will I be deterred from expressing my opinion, by the bold assertion and false reasoning which are so apparent in the following passage:— "Mr. Hunter's opinions may be denied but cannot be refuted. It is however, easier to maintain a proposition diametrically opposite to truth, than one originating in any intermediate degree of error. Those *therefore*, who attempt to account for the vital phenomena upon any other supposition than the one I have had the honour of advocating in your presence, would lead us into such a maze of absurdity, that reason and common sense forbid us to follow them."* The author admits, that Mr. Hunter's opinions, " originate in an intermediate degree of *error*," and *therefore*, any attempt to do away with this *error*, " would lead us into such a maze of absurdity, that reason and common sense forbid us to follow them." " Mr. Hunter's opinions may be denied, but cannot be refuted;" but the author admits that these opinions " originate in an intermediate degree of *error*." In a subsequent part of his physiological lectures, he applies to the gentlemen of his class for a *verdict* in the cause of " Hunter *versus* all opponents." My verdict is *felo de se*. " They originate in an intermediate degree of *error*." The passage quoted is not the emanation of Mr. Abernethy's cool and unbiassed judgment; and I have great pleasure in recording sentences contained in the same work, which do honor to his heart as a Christian, and to his head as a philosopher." How seldom do we consider propositions with the deliberate exercise of those powers of observation and reason with which we are endowed. We are *partial* to our opinions, and consequently *prejudiced* against those of others. In argument we strive for victory, rather than seek for truth. That self-conceit which leads us chiefly to attend to our own opinions and feelings, and to disregard those of others, renders us indocile, unfits us for patient and impartial consideration, and is a great obstacle to the progress of science, as well as to human happiness in general."* "Should the result of our general inquiries, or attention to the subject proposed to us by Drs. Gall and Spurzheim, eventually induce us to believe, that the peculiarities of our feelings and faculties, were the effects of variety of excitement, transmitted through a diversity of organization, they would tend to produce mutual forbearance and toleration. We should perceive how nearly impossible it must be, that persons should think and feel exactly alike upon any subject. We should not arrogantly pride ourselves on our own virtues and knowledge, nor condemn the errors and weakness of others; since they may depend upon causes, which we can neither produce, nor readily counteract."*

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* On the Blood.

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* Abernethy's Physiological Lectures.

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* Abernethy's Physiological Lectures.

All that we know, or that it is essential for us to know, respecting the nature and properties of the blood is this,—that it is composed of certain principles derived from our food and the atmosphere; which principles have, by the peculiar actions of the living system, been converted into component parts of that fluid which we denominate blood; that these principles, in consequence of the changes which they may have undergone in the system, are either convertible to the purposes of the animal economy, or are become useless and excrementitious, in consequence of those purposes having been already accomplished; that the structure and action of the system are immediately influenced by the former, and that the latter are again re-united with the mass of blood, for the purpose of being ejected from the animal frame. We know the sources from whence the blood is derived, can trace it through its several changes, and ascertain the various purposes which it is intended to answer in the animal frame. We should rather congratulate ourselves upon knowing so much, than lament the weakness of our intellects, which will not allow us to know more. We cannot satisfactorily account for the red colour of the blood; but we know that this is a property of minor importance, because we find, that in whole classes of animals the blood is colourless, and still available for the purposes of the living frame. We cannot satisfactorily account for the coagulation of the blood in a state of quiescence, and when removed from the system; but we know that it does not coagulate in the course of circulation and in its healthy state. "Sed maxima fuit questio, unde nam orirentur mirabiles illæ hactenus recensitæ sanguinis proprietates, rubedo illa, seri flavedo, concretio adeò facilis. Philosophi quam mira dixerunt! Quam vana protulerunt nimis sœpè in similibus imprudentes chemici! Nullus interim potuit ullâ arte, vel ex optimis cibis facere ne quidem unam sanguinis guttulam; solum CORPUS HUMANUM suum sibi sanguinem facit ex materiâ, quæ non est sanguis."*

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* Abernethy's Physiological Lectures.

The strict analogy between our food, as contained in the stomach, and the blood, as it exists in the containing vessels, is too remarkable to pass unnoticed. Certain parts of our food are capable of being converted into chyle, and others, not calculated to undergo this peculiar change, are useless, and therefore, at the first convenient opportunity, removed from the system. Certain parts of the blood are capable of being converted into animal matter, and others, not calculated to undergo this peculiar change, are conveyed to certain organs, for the purpose of being eliminated from the system. These parts, after having been subjected to the action of the several organs destined to produce these peculiar effects, are denominated the excretions of the body. Consistently with this view of the subject, the blood may be termed, not metaphorically but positively, " Pabulum vitæ"—the *food* of life. I shall proceed in my endeavours to shew, that the blood must be *digested*, before the ultimate object of its formation can be said to be accomplished.

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* Van Swieten's Comment.

CHAPTER VII.

ORGANIZATION—SECRETION.

THE component parts of the blood derived from the sources already mentioned, are capable of being converted into those substances which constitute the different secretions and excretions of the body.

Mr. John Hunter, as already stated, considered the blood to be a living fluid, and imagined that, in consequence of this property, it was capable of converting itself into the different products which are derived from it. " I have reason to believe," says he, " that the coagulum has the power of action, under necessary circumstances, to form vessels in and of itself; for although the coagulation is not organic, it is still of a peculiar form, structure, or arrangement, so as to take on a necessary action, which, I should suppose, is somewhat similar to muscular action. I think I have been able to inject, what I suspected to be the beginning of a vascular formation in a coagulum, when it could not derive any vessels from the surrounding parts. And again, I have reason to believe, that the blood has the power of action within itself, according to the stimulus of necessity, which necessity arises out of its situation."* But still he admits, that without the previous action of the blood vessels, the blood could not possess that property which enables it to form itself into bone, muscle, membrane, &c. "I would not," says he, "call them arteries; they are the workers—they are the labourers. He would not consider them as concerned in the circulation of the blood—they are the chemists and the architects of the body. That such vessels could separate from the blood, something that was formerly present in that fluid, might be easily credited. But how they could prepare that extreme diversity of fluids and substances, which they are known to do, formed a problem that was never solved till the time of Hunter. He thought that the vital principle of the vessel, acting in that of its fluid contents, produced these chemical changes."† It is the *effect* produced by this "vital principle of the vessel acting on that of its fluid contents,"* that we are now called upon to consider; the subsequent action of the fluid secreted belongs to the process of assimilation. It does not appear to me that the nature of secretion is at all elucidated, by our supposing that the *vitality* of the blood assists in the operation. I do not mean to state, that the process of secretion may not be influenced by the peculiar properties of the blood itself; but it still remains to be proved, that vitality is one of those properties. That state of the blood—its fluidity—which is admitted to be essential to the changes which it is destined to undergo in the healthy system, is attributed, by Mr. Hunter himself, to a living action of the blood vessels ; it is clear, therefore, that the vitality of the blood is not, of itself, sufficient to accomplish that peculiar change in it constituent principles, which has been denominated Secretion. If the blood were capable of converting itself into animal matter, bile, or urine, the complicated structure of the several organs, by which these changes are effected, would be wholly unnecessary. I cannot persuade myself, that organs of a peculiar structure have been provided, without having a peculiar and appropriate function to perform ; and I see no reason why these organs may not, of themselves, be capable of effecting those changes in the constituent principles of the blood, which we denominate secretion, without the assistance of a living principle, existing in and regulating the actions of the blood itself.

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* On the Blood. † Abernethy's Physiological Lectures.

The secretions are wholly derived from arterial blood. The action of the liver has been supposed to be an exception to this general rule; but I shall have occasion to shew, that bile is, properly speaking, an *excretion*, not a secretion. I do not imagine that there is any difference in the mode of action, by which certain principles are secreted or excreted from the blood, and therefore I admit, that excretion is in reality a secretion; but I am of opinion, that a material difference exists as to the nature of those principles, which are separated from the blood by this action; and therefore, confining the term to the *products* of this action, I am disposed to denominate the one a secretion and the other an excretion, and to confine the term secretion to that product which is derived from arterial blood alone. Oxygen, in a free or combined state, may be discovered in all the substances which are derived from the blood. Mr. Hunter says, "indeed there are very few secretions which have not some salt;" but I am of opinion, that there is *no* exception to the rule, that oxygen uncombined, or in the form of an acid or of a neutral salt, exists in *all* the substances which are derived from the blood: but I conceive that the oxygen contained in the secretions, is that individual oxygen which has recently been taken into the system; and that the oxygen contained in the excretions, is that individual oxygen, which has previously existed in the secretions, and which having accomplished its appropriate function in the system, has become excrementitious, and therefore united with the excretions, for the purpose of being eliminated from the system. *This* oxygen, does not, I conceive, form a component part of the arterial influence, and therefore I do not imagine that the excretions are derived from arterial blood.*

As the products derived from the blood contain ingredients which are not to be found in the blood itself, we must admit, that, as far as our knowledge goes, the blood must contain, not the ingredients themselves, but the principles from which those ingredients are formed; and we cannot account for the formation of these ingredients, without admitting that these *principles* must have entered into new combinations, constituting the ingredients which are produced. It may be said, that the different *ingredients* may unite in those proportions, which constitute a substance, so different in its characters from each ingredient considered separately, that the difference between the product formed, and the ingredients which have entered into its formation, is no proof, that any thing farther than a simple union of these ingredients has taken place. If, for instance, an acid unite with an alkali, the neutral salt may not partake of the character of either. This is a fact, which cannot I conceive be determined; and fortunately, it is of no consequence to the argument by which I hope to establish the fact, that secretion is a process similar to that of digestion. If this explanation be applicable to secretion, it is equally so to digestion. Secretion then may be defined to be a disunion and re-combination of the several principles contained in the blood. Digestion has been termed, "the chemical wonder of nature;" the same definition will equally apply to secretion. But there is a wide chasm to be filled up, before we can bring the laws of chemical and living affinity into any thing like a state of approximation. Professor Hufeland says, "Although the powers of animated nature may be *influenced* in many respects by mechanical or chemical properties, yet they are not to be confounded with these, since their laws of action, and their various operations, cannot be explained by any of the principles of mechanics or chemistry which are hitherto known, and appear very distinct from the laws which govern inactive matter."* I am inclined to hope, that the recent discoveries in galvanism, (I allude to its property of counteracting in an extraordinary manner all the known chemical affinities,) and the electro-chemical theory which has resulted from it, and which is now so generally adopted, will bring the laws of chemical and living affinities into a nearer state of approximation at least, and perhaps, establish an identity between them. Mr. Abernethy says, " I consider it already *demonstrated*, that they are electrical actions which cause the various combinations and decompositions so constantly occurring in all parts of living bodies, and upon which their formation and functions greatly depend."†

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* I mean that they are not derived from that *particular* part of arterial blood, which gives to it its characteristic properties.

As the different secretions are in their nature and properties widely different from each other, it is reasonable to suppose that they are derived from different parts of the blood; and it must be admitted, that animal chemistry seems to have thrown some light upon this interesting subject. We learn from a variety of chemical experiments, that a certain substance denominated fibrine exists in the chyle, in the blood, and in animal matter; and it is reasonable to suppose, that the fibrine which exists in the chyle, and in the blood, is the origin of the fibrine existing in that animal matter which is clearly derived from these two sources. This is a strong argument in opposition to the idea, that the blood, in its conversion into animal matter, is in reality *digested*, such a process appearing to be unnecessary; but how does it happen, that all the nutrient arteries do not deposit fibrine? or, admitting that they do so, how does it happen, that one separates a substance readily convertible into bone, another into ligament, another into cartilage, and another into membrane? We cannot suppose that the nutrient substance deposited is in all instances the same, because that implies a waste of substance and an increase of labour, which we never observe in the manufactory of nature.* By what process do these different arteries separate from the blood, in one instance, those substances which are calculated to enter into the composition of bone, in another the ingredients of ligament, and in another the component parts of muscular fibre? Dr. Barlow says, " I know no term so applicable as that of *secretion*, for expressing the office performed by the extreme terminations of the arteries, which convey to each structure of the body its appropriate elements; and although no complicated organ is engaged in performing this office, yet, by the justest analogy, each termination of an artery, so employed, may be considered as a gland, and the term *secretion* applied to express the function it performs. Thus are bones, muscles, tendons, nerves, the brain, the heart itself, all *secreted* by the nutrient vessels of each; nay, the arteries are again themselves supplied by finer blood vessels, appropriated to their nutrition, capable of being demonstrated by injection, and not unfrequently the agents in producing disease."*

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* Med. & Phys. Journ.

† Physiological Lectures.

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* I use the term metaphorically.

Those substances which are derived from the blood, have been termed secretions, but the word must not be accepted according to its strict etymology. Secretion (a secernere, to separate) has been defined, "the separation of one fluid from another in the body of an animal or vegetable;" but this mechanical idea of secretion is, I conceive, wholly inadequate to the explanation of the phenomena we witness. We can discover nothing in the blood, which bears the least resemblance to bile or urine; we cannot therefore suppose, that these substances are merely separated from that fluid.

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* Ed. Med. and Surg. Journ. Vol. 9.

Some writers, in their attempts to explain the actions of the living system, have considered the several organs of the body to be so many mechanical strainers. Baron Haller says, " those who consider that in the blood are found a coagulating serum, an exhaling water, a sort of viscid mucus, and lastly a thick and thin oil, may thence begin to perceive, the possibility of a separation to be made from the blood of all the foregoing classes of humours, inasmuch as we thus see the *constituent principles* are already in the mass of blood itself."* " The lights of these lateral or secreting orifices, though of different diameters in different parts, are yet always small enough in their healthy and natural state to refuse the red blood."† " Some share of the secretion ought perhaps to be allowed to the angle which the secretory branch interrupts with its trunk."‡ " Something also may be attributed to the inflexions of their smaller vessels, greatly retarding the motion of the blood."|| It may be observed, that Baron Haller does not say, the ingredients, but the "constituent principles" are already in the mass of blood itself. These constituent principles, if merely separated from the blood, would remain what they were before. The different principles of bile, in their uncombined state, are as different from the bile itself, after they are separated from the blood, as they were before; these principles therefore, in order to constitute bile, must not only be separated from the blood, but must re-combine in those peculiar proportions, which constitute that animal fluid. Hoffman says, "Diversitas secretionis notissimè a diametro et amplitudine canalium majori vel minori deducitur."*

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* Physiology. † Ibidem. ‡ Ibidem. || Ibidem.

To the mechanical succeeded the chemical theory of secretion. Dr. Henry says, "The solids and fluids thus produced, are sometimes elaborated by complicated organs called glands, and are then termed secretions. A sufficiently exact and comprehensive knowledge of the business of secretion would have been attained, if we were able to discover, in the secreted solids or fluids, substances analagous to those which are found in the blood; but in many secretions we find principles having *no resemblance* to albumen, gelatine, or fibrine, or any of those fluids which form *the proximate elements* of the blood. In these cases nature must have gone further in the work of separation, and after *dis-uniting* the ultimate principles of the blood have *re-combined* them in a new manner, and in new proportions."*

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* Hoffmansii Opera.

To the chemical succeeded the electrical theory of secretion. According to Dr. Valli, " The power by means of which the different organs of the body separate different substances from the mass of fluids, is a mystery in physiology. This power, whatever it may be, is, in our author's opinion, regulated and governed by the nerves. Every nerve, possessing a specific sensibility of its own, is only excited by the contact of substances, which are in relation to it. At this excitement, they impel into action the machine upon which they are expended, and which they govern. The sudden change which the secretions undergo, particularly in consequence of the passions, our author thinks can only be owing to the nervous influence. If, says our author, it were now to be asked, by what means the nerves act, we could only observe, that they act by means of a matter, the subtilty and velocity of which are exceeding great, and that this matter can only be *electricity*."†

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* Henry's Chemistry. † Dunc. Med. Comment.

It must be admitted, that the discoveries of Galvani, Volta, and others, have not produced that sudden revolution in the science of medicine, which was anticipated by these celebrated philosophers, when the phenomena of galvanic electricity were first observed; but it appears to me, that the progress towards that revolution is clearly discernible, is every day more apparent, and I am sanguine enough to expect, that it will ultimately be complete. A considerable period of time has now elapsed since the early experimentalists, led away by the ardour of their imaginations, and yielding to that enthusiastic spirit of discovery, which too often dazzles but to deceive, boldly asserted that the galvanic influence was life; but man, though often led away by his feelings, is a rational animal, and he soon discovered that the galvanic fluid was *not* life. We all know that if a pendulum be impelled to a certain distance on one side of its centre of motion, it will, by its natural re-action, return to the same distance on the other; so has it been with the galvanic philosophers; the galvanic fluid was not *life*, and therefore it was *nothing*; galvanism did not *regulate* the actions of the system, and therefore it had *nothing* to do with them. But the vibrations of the pendulum are contracting upon their centre, and we are gradually withdrawing ourselves from the extremes of enthusiasm on the one hand, and of despair on the other. We now admit that galvanism is neither life, nor the comptroller of living action, but at the same time feel convinced, that it has an intimate connection with both.

Mr. Abernethy says, "I consider it already *demonstrated* that they are electrical actions, which cause the various combinations and decompositions so constantly occurring in all parts of the body."*

Sir Everard Home says, "By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid and the salts of the blood; that with one degree of power, albumen is separated in a solid form; with a less degree it is separated in a fluid form." "Such decomposition of the blood by electricity may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power." "The discovery of an electrical power which can separate albumen from the blood in a fluid state, may explain the mode in which different animal solids and fluids may be produced; since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed."*

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* Physiological Lectures.

Dr. Young has attempted to inquire a little more minutely into the nature of secretion; with what success I leave my reader to determine. "We may imagine, that at the subdivision of a minute artery, a nervous filament pierces it on one side, and affords a pole positively electrical, and another opposite filament a negative pole; the particles of oxygen and nitrogen contained in the blood, being most attracted by the positive point, tend towards the branch which is nearest to it, while those of the hydrogen and carbon take the opposite channel: and that both these portions may again be subdivided, if it be required, and the fluid thus *analysed* may be *re-combined* with new forms, by the re-union of a certain number of each of the kinds of minute ramifications. In some cases the apparatus may be somewhat more simple than this, in others perhaps much more complicated; but we cannot expect to trace the processes of nature through every step; we only inquire into the general direction of the path she pursues, as much in order to avoid being led away by false opinions, as for the sake of any direct advantage that can be gained from our partial views of the true state of the operation."*

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* Philosoph. Transact. 1809.

In our attempts to explain the actions of the living system, we are too much in the habit of admitting nominally, but of neglecting in reality, the important distinction between living and dead animal matter. Dr. Ontyd says, "Electricity is, without dispute, one of the most powerful stimulants of the animal frame; but as soon as it unites with the living power, it is divested of some portion of its natural properties, by means of which it follows the laws of animo-chemical matter, and becomes *animal* electricity."†

I may observe, that the mechanical, chemical, and electrical theories of secretion, all agree as to the point to be explained; they differ only in suggesting various modes, by which the principles of the blood may be "dis-united and re-combined in a new manner and in different proportions." We seem then to admit, that secretion is a dis-union and re-combination of certain principles contained in the blood. What do we know of digestion, or rather, what is the general opinion entertained as to the nature of this action? Digestion is considered to be a disunion and re-combination of certain principles contained in our food. If the process of digestion consist in dis-uniting the principles of our food, so that they may re-combine in those peculiar proportions which constitute chyme, chyle, or blood ; in what respect does it differ from the process of secretion, by which these same principles, (for we must never forget that the blood is simply our food and air in a peculiar state of combination,) may be dis-united so that they may re-combine in those peculiar proportions which constitute animal matter. The organs of digestion act upon the principles of alimentary matter before their conversion into chyme or chyle ; the organs of secretion after their conversion into blood. The organs of digestion and of secretion are acting upon the *same* substances : they are equally employed in enabling the principles of our food and air to enter into that peculiar combination which is the ultimate object of our taking them into the system, viz. the organization of living animal matter ; and I am inclined to suppose, that there is no difference in the mode of action by which these several progressive changes are effected. Mr. Hunter says, that our food is animalized in the stomach ; Dr. Rush imagines that it undergoes this change, after its conversion into blood. The latter author says, "The design of the liver, I believe to be, to receive the blood from every part of the body, in order to subject that part of it which had not been completely *animalized* or divested of its chylous properties, to a *secretory* process, and afterwards to pour the products of this secretion, mixed with the liquor of the pancreas, into the duodenum, to be absorbed, or otherwise taken up by the lacteals, and conveyed with the chyle from the stomach into the blood vessels, in order to be completely converted into red blood, for the purpose of serving the various and important uses for which that fluid is intended in the human body."* An identity of effect is here attributed to the action of the stomach, and of a secreting organ. The liver is in reality considered to be a secondary stomach.

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* Med. Literature. † Med. & Phys. Journ.

We do not deny that the food of vegetables is digested. The process of digestion, as performed by the stomach, is wholly deficient; and yet the organization of vegetable matter is as perfect in its kind, as that of the animal frame. That change which is necessary to the conversion of the *food* of plants into an assimilation with their peculiar structure, is performed by the action of the absorbents of the blood vessels, (so called from their analogy with those of the animal frame,) or of the secreting organs. Digestion is here performed without a stomach.

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* Med. & Phys. Journ. 1806.

In some *animals*, the alimentary canal is not distinguishable into œsophagus, stomach, or intestines. It is not improbable, that their digestion approaches very near to that of vegetables; at least we have no difficulty in perceiving, how an animal may digest its food without a stomach. Digestion then may be regarded as a process of secretion, and secretion as a process of digestion. I conclude therefore, that they are the same things in effect, and I shall endeavour to shew, that they are the same things in action.

CHAPTER VIII.

ORGANIZATION—EXCRETION.

I HAVE already said, that Excretion is a secreting process. "The term secretion, as generally understood, applies to the office of certain organs termed glands, which separate from the blood conveyed to them, each its appropriate fluid. Thus bile is a secretion of the liver, urine of the kidnies." "The larger glands however, seem destined rather for the purposes of excretion, and the various organs by which this is effected come next to be considered. As their office, however, is that of separating from the pure and nutrient blood, those particles which *having performed their part*, are no longer serviceable, and of expelling them from the body, the order of nature leads us here to notice that process, and those organs by which the effete particles are re-conveyed into the circulation, preparatory to such expulsion ; these are the lymphatics or absorbents—a system of vessels which pervade every part of the body to which blood is conveyed, and whose office is to take up, and carry back to the circulation, all matter that is no longer serving an useful purpose, as soon as it shall be reduced to a state fitted for undergoing their operation."*

An important distinction, (more particularly as connected with the pathology of the system,) between secretion and excretion, is here suggested. I have already had occasion to give my reasons for considering the lacteal and lymphatic vessels as one system of absorbents, and at the same time pointed out the difference in the nature of the substances which they severally convey to the mass of circulation; the former acting upon those substances, which have not yet answered the purposes of their being connected with the body, the latter upon those which have already accomplished their destined functions in the system. If therefore, in considering the object of secretion and excretion, we go one step farther back, we shall connect the lacteals with those organs, which accomplish the former function, and the lymphatics with those organs which are to be considered, as strictly speaking, excretory. If it were possible to determine from what parts of the blood the different secreted and excreted substances are derived, a fact of great practical importance might, I conceive be established.

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* Dr. Barlow.

I have already shewn, that the chemical analysis of the blood, and of the different secreted and excreted substances, has thrown scarcely a gleam of light upon this obscure but interesting subject. "But in many secretions we find principles having *no resemblance* to albumen, gelatine, or fibrine, or *any* of those fluids which form the proximate elements of the blood;"* we cannot therefore expect to derive much assistance from chemistry.

Unacquainted as we are with the nature of the blood itself, and of those substances which are derived from it, we cannot hope to arrive at that degree of certainty, which might enable us to say positively, that one substance is secreted, and another excreted, from the sanguineous mass. We know that the blood consists of certain substances derived from our food through the medium of the lacteal absorbents, and from the surrounding atmosphere through the medium of the respiratory organs; these I conceive to be essential to, and to be intended to form a component part of, the living organization.

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* Henry's Chemistry.

The nutrient fluid therefore, which is elaborated "by the extreme terminations of the arteries which convey to each structure of the body its appropriate elements," is strictly speaking a secretion. It is not reasonable to suppose, that the chyle which has been so industriously elaborated in the complicated process of digestion, can be conveyed to the blood for the purpose of being immediately eliminated from the system; the ingredients of the chyle therefore, we must presume to be contained, not in the excreted, but the secreted fluids of the system. As a constant supply of oxygen is found to be necessary to the continuance of living action, and as the necessity of a constant supply implies that a constant expenditure is going on, we presume that the oxygen which is united with the blood through the medium of the respiratory organs, is contained, not in the excreted, but in the secreted fluids. If we suppose the fact to be otherwise, we must either imagine, that the oxygen serves no other purpose than that of uniting with the blood, (an idea which is wholly inadequate to the explanation of those phenomena which accompany living action,) or that an unnecessary expenditure of this important principle is taking place. It appears to me, that the phenomena of living action cannot be explained, without admitting that the oxygen inspired becomes a component part of the living organization; we presume therefore, that this principle is contained in the nutrient fluid secreted by the extreme terminations of the arteries.

We find that certain substances are separated from the blood apparently for the express purpose of being eliminated from the system; thus the insensible perspiration communicates immediately with the external atmosphere, and the urine is evidently separated from the blood, and conveyed to the bladder for the same purpose. No advantage to the system can be supposed to be derived from its detention in this organ; nature, in this instance, seeming only to have consulted the convenience of the animal. We cannot suppose that any substances, which are capable of contributing to the welfare of the system, are contained either in the insensible perspiration or the urine; they are therefore to be considered, as strictly speaking, excretions. The idea, that gaseous substances are perspired by the skin, was first suggested by Count de Milly. "It has been the observation, he remarks of a celebrated modern, that the *ennui* may even contribute to our knowledge. In this state he had occasion to observe, in the bath, while he lay at rest, a prodigious quantity of small silver-coloured air bubbles, resembling what are called the seeds of pearls, on the surface of his body; in a short time they became larger, and at last, on the slightest agitation, detached themselves from their base, rose to the surface of the water, burst, and disappeared. Struck by this interesting appearance, he formed the design of collecting this aëriform substance. A glass decanter, which served for holding lemonade, and which he had by him, was the first instrument which he employed. This he filled with water, and having inverted it, he held the mouth of it above those parts where the air-bubbles were most numerous; he then gently touched the air-bubbles with his other hand; these instantly rose to the surface of the water contained in the inverted decanter."* This gas was examined by Lavoisier, and pronounced to be carbonic acid; but the accuracy of this conclusion was questioned by Drs. Ingenhouz and Priestley. It was inconsistent with the opinions which the former entertained as to the nature of vegetable respiration, but the late experiments of Mr. Ellis have, in my opinion, satisfactorily proved, that the air undergoes precisely the same changes, whether it be subjected to the process of vegetable or of animal respiration; oxygen is lost, and carbonic acid generated. The subsequent observations of Cruickshank, Spallanzani, and Abernethy, appear to me to have confirmed the experiment of Count de Milly, and to have verified the conclusion of Lavoisier. "Mr. Cruickshank, when engaged in some experiments on the insensible perspiration, found that the air, in which his hand had been for some time confined, was vitiated; that a candle burned dimly in it, and that when washed with *lime water* a *precipitation* took place.*" "The last experimental philosopher, who has paid particular attention to this subject is Spallanzani. The detail of his experiments has not been published; but the results are preserved in a letter to Senebier, introductory to his memoirs on respiration. He appears to have been completely satisfied, that air in contact with the skin, is changed exactly in the same manner as by respiration. When atmospherical air was employed, oxygen disappeared, and carbonic acid was produced; but he is of opinion, that the oxygen of the atmosphere had no share in the production of the carbonic acid gas, because it was equally great when the animal was confined in gases containing *no* oxygen: and he therefore concludes, that the carbonic acid gas was exhaled, and the oxygen absorbed."* "Mr. Abernethy's experiments were performed by introducing his hand and wrist under a glass jar inverted in the mercurial trough.

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* Dunc. Med. Comment. 1786.

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* Ed. Med. & Surg. Journ. 1805.

1st, The jar being filled with quicksilver, he collected, in the space of sixteen hours, one half ounce measure of air, which had escaped in small bubbles, from every point of the surface of the immersed hand. Two-thirds of this gas were absorbed by lime water; the remainder suffered no diminution by the mixture of nitrous gas.

2nd, The experiment being repeated under water, a small quantity of azotic gas only was obtained, the carbonic acid having been, in the opinion of Mr. Abernethy, absorbed by the water.

3rd, The jar contained seven ounces of atmospheric air. After five hours' exposure of the hand, the air in the jar was diminished about half an ounce. Of the remaining air one ounce was absorbed by lime water, with precipitation of the lime. By the test of nitrous gas, the air was found to contain nearly one-sixth part less of oxygen gas than it did before the experiment.

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* Ed. Med. & Surg. Journ. 1805.

4th, The jar contained seven ounces of azotic gas. At the end of two hours, rather more than one ounce measure of carbonic acid gas was produced.

Similar experiments were made with hydrogenous, nitrous, and oxygenous gases; and in every case the quantity of carbonic acid gas obtained was nearly the same. "Experiment shews then," says Mr. Abernethy, "that carbonic acid is *perspired* from the vessels; for into whatever air the hand be immersed, the quantity of carbonic acid given out will be nearly the same."*

I may appear to my reader to have been rather too liberal in my quotations on this subject; but the point to be determined, is one of great *practical* as well as physiological importance, and the experiments recorded, although apparently so decisive on the subject, have not carried conviction to the minds of all. "Adhuc sub judice lis est." The only difficulty suggested is this:—is the carbonic acid *perspired* from the skin, or is the carbon only perspired in a state to unite with the oxygen of the external atmosphere. The fact, that in every case, whether the experiments be made with hydrogenous, nitrous, or oxygenous gas, the quantity of carbonic acid produced is nearly the same, appears to me to set this point at rest. But we have ascertained that oxygenous gas does disappear. How is this fact to be explained, without admitting that it enters into combination with the carbon ? is it inspired by the skin ? I have already attempted to prove, that this action of the skin is very improbable. We must remember, that the sensible and insensible perspiration, are not composed of carbonic acid gas *alone;* there is a fluid, in a solid or gaseous state, which enters into their composition ; why may not the oxygen lost unite chemically or mechanically with this *fluid?* Why may it not unite with the fluids of the skin, and thus become the legitimate object of lymphatic absorption ? Upon the whole, I consider the fact, that carbonic acid is *perspired* by the skin, to be established by the experiments of Abernethy and Spallanzani.

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* Ed. Med. & Surg. Journ. 1805.

We all admit that the carbon must be derived from the system, because we cannot possibly trace it to any other source, and consequently we admit, that carbon must form a component part of the animal frame. As the organization of the system is evidently derived from the chyle, and from the oxygen of the air (which last contains no carbon), we are under the necessity of admitting, that chyle must contain either the ingredient itself, or the principles from which the carbon is formed. It is not reasonable to suppose, that the carbonaceous principles contained in the chyle, have been prepared from our food for the purpose of being immediately ejected from the system; and therefore we presume, that the carbonic acid is not derived from the principles contained in the chyle. But the organization of the system, is constantly undergoing that change, by which its individual particles lose their vitality, and are then taken up by the lymphatics. We presume therefore, that the carbon, which is taken up by the lymphatics, and not that which exists in the chyle, is the individual carbon contained in the perspired carbonic acid. The same arguments apply to the oxygen; and therefore we presume, that the oxygen contained in the carbonic acid perspired, is not the individual oxygen which has *recently* been taken into the system, but that which has already formed a component part of the living organization, and which, having accomplished its destined function in the system, has bebecome excrementitious. The perspiration therefore, is, strictly speaking, an excretion, and not a secretion.

For the same reasons, the carbonic acid expired must be considered as an excretion. It has been supposed, that the oxygen of the carbonic acid expired, is derived from the atmosphere; and the fact is imagined to be proved by the alleged circumstance, that the quantity of carbonic acid expired, exactly coincides with the quantity of oxygen inspired; but this proves too much, because arterial blood, from whatever part of the system it is drawn, presents those characters which the same physiologists attribute to the presence of oxygen; it could not be contained in the carbonic acid expired, and also exist in the blood; I conclude therefore, that the oxygen inspired has nothing to do with the carbonic acid which is eliminated from the system, in the expiration immediately succeeding the inspiration; and I also conclude, that the oxygen contained in the carbonic acid expired, is that which, having traversed the system, has become excrementitious. Expiration therefore, is an excretory, and not a secretory process.

According to Mr. Hunter, all secreted (in which he includes excreted) substances contain some salt, that is, some base combined with an acid, and that implies the presence of oxygen. The urine contains a variety of salts; but for the reasons before mentioned, the oxygen which enters into their composition, I conceive to be that which has already traversed the system, and become excrementitious. The urine therefore is, strictly speaking, an excretion and not a secretion. It may sometimes happen, that a greater quantity of oxygen is inspired, than the necessities of the system require. The explanation of the phenomena dependent on this circumstance, is highly important in a pathological point of view; but I should clearly anticipate my subject, by entering upon it at present.

In my observations upon the nutrient fluid and the insensible perspiration I have taken the two extremes, and I think it will readily be admitted, considering the subject rationally, that the substances contained in the former, are conveyed to the blood by the lacteal absorbents and the respiratory organs, and that those contained in the latter, are conveyed to the mass of circulation through the medium of the lymphatic vessels; that the former are derived from the useful, and the latter from the excrementitious parts of the blood. There are certain intermediate secretions, the origin of which cannot be easily demonstrated; but I think it will readily be granted, that all those substances which are deposited in cavities, that is, in situations from whence they cannot be removed except through the medium of the lymphatic absorbents, are, strictly speaking, secretions; and I presume this to be the fact, because, to imagine otherwise, is to suppose, that the same substances may be employed *ad infinitum* to the same purpose, or that the same substances are employed to answer different purposes in the animal economy; an idea, which appears to be inconsistent with the bountiful supply of those principles which are found to constitute the component parts of the living organization. I presume therefore, that the synovia of the joints, the exhalations from internal membranous and mucous surfaces, and many other substances corresponding with the definition given, are in reality secretions, and not derived from the excrementitious portion of the blood. But there are some substances, which are separated from the blood and poured into cavities immediately communicating with the external atmosphere, and which on that account might be considered as excrementitious; but at the same time they are found to answer such important purposes in the animal economy, that it is not easy to regard them in this light: I allude to the bile, the pancreatic fluid, the gastric fluid, and the saliva: these are all instrumental in the process of digestion, and yet appear to be removed from the system, without being taken up by the lymphatic absorbents. It is certain that in disease, the bile is very often absorbed; but it may be questioned, whether this absorption take place through the medium of the lacteal or lymphatic vessels. I have already shewn it to be very improbable that, in a state of health, the bile is taken up from the cavity of the intestines by the lacteals, and therefore I should be inclined to regard the bile as an excretion; but when we consider the important uses of the gastric fluid, can we imagine that it is derived from the excrementitious parts of the blood? I believe that the process of digestion has been too much attributed to the agency of the gastric fluid, and too little to the action of the stomach itself. In our reasonings upon the nature of this action, our views have, I conceive, been too mechanical. There is no action in the living system, not even muscular motion, the phenomena of which can be explained upon the principles of mechanism alone. The motion of one joint upon another may be mechanical, but the phenomena attending that motion cannot be explained upon the principles of mechanics alone. I presume for instance, that friction will not account for that generation of animal heat which attends muscular action. I conceive then that the nervous influence is more, and the gastric fluid less, concerned in the process of digestion, than is generally imagined ; and therefore I have no difficulty in imagining that a fluid, possessing the *chemical* properties of the gastric juice, may be generated from the excrementitious parts of the blood. If nature has rendered the excrementitious part of the air subservient to the important purpose of language, to the communication of ideas and feelings between separate individuals, why may we not imagine that a fluid, possessing the *chemical* properties of the gastric juice, may be derived from the excrementitious part of the blood? I see no conclusive fact which should lead me to deviate from the definition I have offered ; and therefore, in consequence of their communication with the atmosphere, independently of any action of the absorbent system, I am disposed to regard the saliva, the gastric fluid, the biliary and pancreatic juices, as excretions. I am aware, that so much importance has been attached to these different fluids, that my reader will be startled at the idea of the gastric fluid being an excretion; but the line must be drawn somewhere, and I shall endeavour to shew in a subsequent part of these observations, that digestion is not a chemical but a living action, and that the importance attached to the gastric fluid has been very much over-rated. Were we to judge from the magnitude of the organ employed, and the apparent importance of the fluid secreted, we should have no hesitation in saying, that bile was a secretion. The function which I have attributed to the bile—that of precipitating the fœculent parts of our food from the chylous portion—is strongly exemplified in the following passage, which I omitted in the proper place; but some advantage may be derived from recalling the attention of my reader to this part of my subject:—" I am, however, happy to say, that Mr. Cooper, the gentleman already mentioned, has ascertained the fact, and has absolutely *seen* the precipitation take place, by opening the chylopoietic organs during the time that the chylopoiesis was going on."* And yet most authors seem to be of opinion, that the liver has some other important purpose to accomplish, than that of precipitating the fœculent parts of our food. This organ is considered by one to be a secondary stomach, digesting again the food which was imperfectly *animalized;*[*] by another it is considered to be a secondary respiratory organ. Professor Cuvier says, "Thus the considerable magnitude of the liver in those animals which respire least, and its total absence in insects, which possess the most complete kind of respiration, since their whole body forms, as it were, their lungs, have induced us to conclude, that the liver, in a certain degree, supplies the place of lungs, and like them serves to remove from the blood its two combustible principles." It is clear that the author here alludes to the excretory action of the lungs, the expiration of "the two combustible principles"—oxygen and carbon. This idea is also expressed in another part of the same work. "The matter which transpires through the pores of the skin, and the substances which pass through the intestinal canal, a great part of which are carried away with the excrements, *relieve* the blood of other principles."[†] I do not agree with this author, in supposing that insects have no liver, because they have the most perfect respiratory organs; they breathe with their whole bodies, it is true; but surely the extent of surface which the numerous air cells of the human lungs present to the atmosphere, must be greatly superior to that of the surface of the body. I should say, that the respiratory organs of insects are, comparatively speaking, imperfect; that a smaller quantity of air is taken into the system, and therefore a smaller quantity is vitiated, and that the excretory action of the liver is, for that reason, unnecessary. Dr. Barlow agrees with Professor Cuvier, in considering the liver as an excretory organ; but he is led to this opinion by a different process of reasoning:—"Although the uses already stated have been those generally assigned to the liver, yet doubts have at all times existed on the subject; and it has been argued, that the size of the organ seems disproportionate to such an end; that the blood from which bile is secreted, is not such as is supplied to any other gland or secretory; that experiment has shewn, that the bile, as it exudes from the hepatic duct, does not mix with the chyme, but merely diffuses itself over the surface, and the coats of the duodenum; that the bitterness of bile seems to have no agreement with the blandness of chyle; and that in jaundice, when its flow into the intestines is completely interrupted, although costiveness supervenes, nutrition does not seem to be suspended. All those arguments may be deemed vague and inconsequential, but the frequency with which they have been urged, serve to shew, that physiologists have not been satisfied with the opinions generally entertained. My own belief is, that the principal office of the liver is that of serving as an *excretory* from the system; and though I am not prepared formally to prove this by an induction from facts or experiments, I shall try to justify my faith, by stating such arguments, as an attentive observation of its diseased actions has hitherto furnished me with."* We here find two authors pursuing a different course of reasoning, and arriving at the same conclusion; and that conclusion is, that the liver is an excretory, and not a secretory organ. To this may be added the singular fact, that bile is derived, not from arterial, but from venous blood; that is, from blood which has already circulated through the system, and which, therefore, we may presume to have accomplished its destined function in the system. It may be said, that the peculiar function attributed to the liver by Professor Cuvier is wholly unnecessary, because the quantity of carbonic acid expired by the lungs, exactly corresponds with the quantity of oxygen inspired. But we must remember, that the lungs are not the only sources of oxygen to the system. A considerable portion appears to be constantly conveyed to the stomach in union with the saliva; "Aër, qui magnâ copiâ in ventriculum descendit inclusus vesiculis salivosis;"* and our food, (particularly the fluid ingesta) contains a very considerable portion of this principle. I shall conclude this part of my observations by saying, that I cordially agree with those authors who have regarded the bile as an excretion. Secretion therefore, may be defined to be that substance which is separated by the action of a living organ from the nutrient part of the blood; and excretion may be said to be that substance which is derived, through the same medium, from its excrementitious portion; but secretion and excretion, as far as the living action of the organ is concerned, may be regarded as the same things.

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* Saumarez' Physiology.

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[*] I have already quoted the opinion of Dr. Rush as to this function of the Liver.

[†] Cuvier's Compar. Anat.

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* Ed. Med. & Surg. Journ. 1813.

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* Haller.

CHAPTER IX.

ORGANIZATION—NUTRITION.

WE have already seen that Nutrition is a process of secretion; but the nutrient fluid must not only be prepared by the arteries—by the labourers—but must be assimilated with the corresponding parts of the living organization. We cannot hope to follow nature into the recesses of her laboratory; we may see a substance go in a globule of blood, and come out a piece of bone; and we may *imagine* the different changes which this substance has undergone. The most simple view of the subject appears to be this:—The fluid secreted in the neighbourhood of bone, possesses a greater aptitude, to be converted into that substance, than into any other component part of the living organization; and this greater aptitude may be supposed to depend upon that fluid possessing a larger proportion of the osseous principles, than what is secreted in the neighbourhood of muscular fibre; these principles may be supposed to unite by a species of elective attraction, and to enter into that peculiar combination which is consistent with the *co-union* of the living principle; they become, therefore, component parts of the living organization; other principles of this fluid may not be capable of entering into that peculiar combination, and therefore do not partake of the living principle, and are consequently taken up by the lymphatic absorbents, for the purpose of being removed from the system. We cannot suppose that the nutrient fluid is precisely the same in whatever part of the animal frame it may chance to be secreted, because this implies an unnecessary waste of those principles which may be capable of assimilating with *other* parts of the body. We presume therefore, that each artery secretes the nutrient substance, in a state possessing a greater aptitude to unite with one part of the animal frame than with another. But each organ has its peculiar form; we may presume therefore, that those particles which do not unite, so as to correspond with the form which nature has assigned to an organ, lose their vitality, and thus become subjected to the action of the lymphatic absorbents, through the medium of which they are removed from the system.

Assimilation then may be divided into three stages: the secretion of the nutrient principles—the union of these by a species of elective attraction with the corresponding principles contained in the organ to be repaired—and the absorption of those principles, which from their want of aptitude to unite with that peculiar organ, or from their misapplication, so as not to correspond with the peculiar form of that organ, do not acquire, or having acquired, do not retain the vital principle.

Almost all our ideas of the nature of assimilation have been deduced from observations made upon the formation of bone. This substance, from the slowness of the operation, and from other circumstances connected with its formation, (I allude more particularly to the fact, that madder may be subjected to the action of the digestive organs, and still retain the property of communicating its peculiar colour to the bones of animals,) has most frequently been the subject of experiment. Dr. Monro says, "The ossification of bones depends principally on their vessels being so disposed, and of such diameters, as to separate a liquor, which may easily turn into a bony substance, when it is deprived of its thinner parts, as seems plain from the observation of the callous matter separated after fractures and ulcers, where part of the bone is taken out."* This idea of secretion is rather too mechanical for the present day. Mr. John Hunter thought, that "the vital principle of the vessel acting on that of its fluid contents, produced those chemical changes."† There is, I conceive, too much of life, and too much of chemistry wrapped up in this opinion. This theory has been modified by his commentator into another, which, with one exception, I am inclined to adopt. "In speaking of digestion, it has been already shewn, that actions productive of secretion are regulated by the general powers of life, by the energies of the nervous system." As I do not consider "the general powers of life, and the energies of the nervous system," to be the same things, I should rather say, "it has already been shewn, that actions productive of secretion, are regulated by the energies of the nervous system." By such an action of the nutrient arteries, I conceive the osseous deposit to be made.

An ingenious author, Dr. Gibson, has commenced his observations upon this subject with the state of the blood previous to this secretion. "It had been some time known to physiologists, that the bones of a young animal fed on madder root acquired a red colour: but Dr. Rutherford was the first who gave any thing like a satisfactory explanation, why the colouring matter was communicated to the bones alone of all the white parts of the body; he conceived that the colouring matter of the madder had a superior attraction to the phosphat of lime; that it united with that part of it which was floating in the general mass of blood; so that all future bone which was formed, had a red colour, from the manner in which the red colour is deposited. Duhamel's opinion of the lamellated structure of bone was thus supposed to receive an additional confirmation, and the alternate appearances of red and white laminæ, produced by feeding the animal with, or depriving it of the madder, were considered as convincing proofs, that the harder parts of the animal frame were constantly undergoing the same waste and repair, which had been generally believed to take place in the other parts. Mr. Gibson, after pointing out the erroneous conclusions to which this doctrine leads, explains the phenomena in a very ingenious and much more satisfactory manner. He found by experiments, that the serum of the blood had a much greater attraction for madder than the phosphat of lime, so that when an animal has madder mixed with its food, the blood becomes charged with it, and the superabundant colouring matter is imparted to the phosphat of lime contained in *the bones already formed*; that as soon as the animal has ceased to receive the madder, and the blood is freed from the colouring matter by the excretions, the serum then exerts its superior attraction, and by degrees entirely absorbs it from the phosphat of lime, and the bones resume their natural whiteness."* Mr. Howship has presented us with a very accurate description of the changes which the *secreted fluid* undergoes in its transformation into bone, and of some circumstances connected with the structure and action of those organs, from whence this fluid is secreted. I find it impossible to give a condensed, and at the same time a satisfactory view of this author's opinions; I shall therefore refer my reader to the work itself; but I may observe that the opinions, which Mr. Howship considers to be original and peculiar to himself, are not at all inconsistent with the view which I have already taken of the process of assimilation.

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* Monro's Works.

† Abernethy's Physiological Lectures.

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* Ed. Med. & Surg. Journ. 1814.

That author says, " The existence of the organization of membranous sheaths in cartilage, and their office in furnishing a peculiar *secretion*, together with the regular arrangement of the foramina upon the ossific surface, and the purposes they are destined to fulfil in the animal economy, are points, which as far as my reading goes, no author has ever mentioned or even glanced at."

It generally happens, that the variety of opinions entertained upon a subject, bears an exact proportion to the obscurity in which it is involved. Such appears to be the case in the present instance; I shall not, therefore, undertake the Herculean task of recording the various sentiments which have been promulgated by a host of authors, from Kerkringius downwards to the present time, upon this obscure, and, if I may use the expression, impalpable subject. The great object of dispute appears to have been, whether the osseous matter be deposited in a laminated, lamellated, or reticulated form; and I think my reader will readily dispense with a tedious and elaborate discussion of this point. Assimilation then may, I conceive, be considered as the result of the following combined actions:—secretion, elective attraction, and lymphatic absorption.

On the subject of GENERATION, (I mean the actual formation of the embryo,) I shall say but little, because a great deal has already been said to no purpose. I imagine that no organized substance is formed without the combined action of secretion, elective attraction, and lymphatic absorption. We often find that the organization of the fœtus is malformed and imperfect; this state of the organization, as applied to particular organs, appears to be corrected by the healthy action of the lymphatic absorbents; we may therefore presume, that when it occurs in the formation of the embryo, it is owing to a deficient action of these vessels. Upon the whole, generation may as well be denominated a process of nutrition or assimilation, as of any thing else.

CHAPTER X.

ORGANIZATION—ELECTRICITY.

"THE word Electricity denotes a peculiar state of which *all* bodies are susceptible, and which is supposed to depend upon the presence of a substance called the electric fluid."* It might almost appear unnecessary to mention, and more particularly to adduce any authority in support of, a fact which is so generally admitted; but it is remarkable that this fact, as applied to the animal frame, although universally allowed and in reality undeniable, has been but little regarded by physiologists.

Huxham kept a register of the weather for twenty years, and occasionally noticed the occurrence of a thunder storm, of summer lightning, and other electrical phenomena; but how seldom does he allude to these occurrences in his pathological observations! Sir John Pringle has prefixed to the history of almost every disease which he describes, an Account of the State of the Atmosphere; and although his theory is principally founded upon the putrescency of the animal fluids, (a condition which is known to supervene almost immediately upon death occasioned by lightning,) the effect of an electrical state of the atmosphere upon the human body is seldom noticed. Our pathological reasonings must always assume the cast of our physiological opinions; the above examples therefore, though apparently applicable to pathology alone, cannot be considered as inapplicable to this part of my subject.

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* Nicholson's Encyclopedia.

We admit that electricity is a principle which has a tendency to pervade all matter; but we find that some substances offer a greater resistance to its passage than others, and certain bodies *appear* to have the power of resisting it altogether. These substances have been denominated good or bad conductors, and non-conductors of electricity. We know from positive experiment and observation, that the animal frame is a conductor of electricity. By placing an individual upon an insulated stool, we can produce artificially an accumulation of electricity in his body; but some persons may be inclined to doubt, whether under ordinary circumstances such an accumulation can take place naturally; the following examples appear to be decisive upon this point:—"In these letters, after recounting several very remarkable phenomena occurring during intense cold weather, he particularly mentions what happened in the case of Count Orloff, and his Imperial Highness the Grand Duke. With the former, Mr. Æpinus found, that at every time his valet-de-chambre drew the comb through his hair, when at his toilet, a strong crackling noise was heard; and on darkening the room by drawing the curtains, the sparks were seen following the direction of the comb in great abundance; whilst, by this operation, the Prince was become so completely electric, that strong sparks could even be drawn from his hands and face."* "After particularly relating these, and several other curious phenomena, which might be considered as striking effects of the electric state of the atmosphere, occurring during a great degree of cold, which had continued for several weeks, Mr. Æpinus enters into much ingenious reasoning, with a view to explain this disposition of the air. The result of all his reasoning is, that after the great cold in Russia has continued for a certain time, not only air, but also silk, hair, wool, and similar bodies, are in fact drier than during the rest of the year, and probably more so than in any other part of Europe, unless they be dried expressly by some artificial means. The consequence of this is, that the bodies above mentioned become spontaneously much better *ideo-electrics* in Russia during the cold weather, than they ever are in any other season or climate; hence these bodies have an extraordinary disposition to become easily and strongly electric."* An equally strong example of the accumulation of electricity in the animal frame is recorded in the following account, communicated to the Royal Society of Edinburgh :—" In the year 1767, M. de Saussure, M. Pictet, and M. Jalabert, ascended to Mount Breven, which is situated nearly in the middle of the valley of Chamouni, and almost exactly opposite to Mount Blanc. Their object was to take a general view of the form and position of the glaciers which descend from Mount Blanc. When they reached the summit, M. Jalabert began to take a drawing of the glaciers; M. Pictet was engaged in the geographical part, and M. de Saussure was preparing to make his experiments on natural and artificial electricity.

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* Dunc. Med. Comment.

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* Dunc. Med. Comment.

When M. Pictet was laying down upon his plan some particular mountains by means of a graphometer, he had occasion to ask the names of them from the guides, and was therefore obliged to point out the individual mountains with his finger. Every time that he raised his hand for that purpose, he felt at the extremity of his finger a kind of tremulous and prickly sensation, similar to that which is experienced upon presenting the finger to a globe of glass highly electrified. M. Pictet soon perceived the origin of this phenomenon. He observed a stormy cloud in the middle region of Mount Blanc, directly opposite to Mount Breven, and it instantly occurred to him, that he had been affected with the electricity of the cloud. He then requested M. M. de Saussure and Jalabert to make the same experiment, and as soon as they extended their hands, they experienced a similar sensation, which they described as resembling a number of electric sparks; but fearing that they might be seduced by their imagination, they made their guides and their servants stretch out their hands, and they felt the same sensation. The electricity of the atmosphere having now begun to increase, the sensation became stronger, and was even accompanied with a kind of whistling noise.

M. Jalabert, who had a gold band upon his hat, was alarmed with a buzzing noise about his head, and having taken off his hat, and put it successively upon the heads of Pictet and Saussure, they heard distinctly the same sound, and even obtained sparks from the golden button of the hat."*

Notwithstanding the length of this quotation, (which I think is justified by the importance of the fact to be established, and the scepticism still evinced by many writers upon the subject which I am endeavouring to illustrate, viz. the connection between electricity and the actions of the living system;) I shall not hesitate to subjoin the account of another journey, which not only tends to prove, that the human body, without artificial means, may be charged with electricity, but seems at the same time to shew, that the *actions* of the system are attended with an evolution of that principle. " Electrical phenomena, much similar to the above, were very recently observed by a party of Englishmen, when they were descending Mount Ætna, during a storm of thunder and lightning. On the 2d of June, Mr. Tupper and Mr. Lanfiar, accompanied by a guide, ascended Mount Ætna. During their descent, when at a little distance from a place called the English House, in the regio deserta, they were overtaken by a storm of thunder and lightning, accompanied with a heavy fall of snow; and while running over an extensive field of snow, they were struck by a flash of lightning, from which, however, they experienced no serious injury. Mr. Tupper felt a painful sensation in his back, which gradually ascended towards his head, and occasioned a sensation as if his hair was moving, an effect exactly similar to that which is produced, either when a person is electrified upon an insulated support, or when his head is presented to a prime conductor of an electrifying machine. This sensation induced Mr. Tupper to raise his hand to his head, upon which he was surprised to hear a buzzing noise, proceeding from his finger. Upon raising and extending his arm, the noise still continued; but upon moving his hands and fingers in different directions, and with different degrees of velocity, he found that he could produce, at pleasure, a great variety of harmonic sounds, differing in their tone as well as in their intensity, and so loud, as that they could be distinctly heard at the distance of forty feet. The Sicilian guide witnessed these phenomena with extreme dismay, and imagining that Mr. Tupper produced the sounds in virtue of some supernatural power, he immediately began to cross himself, and invoked the protection of his saint; his alarms, however, speedily subsided, when, upon being directed to elevate his own arm, he found it as musical as that of Mr. Tupper. Mr. Lanfiar, who was a little behind the rest of the party, now joined them, and found that his fingers possessed a similar property. In the course of five minutes, reckoning we presume from the arrival of Mr. Lanfiar, their fingers lost their acoustic property, the cloud having by this time passed to a considerable distance."* The simple explanation of this fact appears to be, that the animal frame was overcharged with electricity, and that, upon every motion of the fingers, sparks were given out, producing those vibrations in the air, which constituted the harmonic sounds. but, in order to give out these sparks, the animal frame must have been more highly charged than the surrounding bodies; is this fact explained by the fall of snow which was at that time taking place? It is of no consequence to my argument, whether we suppose that muscular motion produces an accumulation of electricity in the organ of motion, so that the sparks pass from that to the surrounding bodies, according to the known laws of electrical matter; or whether we imagine, that the electricity is separated from the organ by its own action; because an increased supply, an increased accumulation of the electric principle, involve the necessity of an increased expenditure; otherwise the equilibrium necessary to health could never be restored. No rational doubt, then, can, I think, be entertained, that the quantum of electricity existing in the human body is different at different times.

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* Med. & Phys. Journ. No. 193.

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* Med. & Phys. Journ. No. 193.

There is also reason to suppose, that the quantum of electricity existing in the various parts of the body is not precisely the same. This fact may be inferred from the well-known phenomena which accompany certain actions of the Torpedo, and Gymnotus Electricus. "On touching this fish, as it lies in the water in a tub provided for it, a sudden and violent shock is received. This is, we are told, in all respects, like that which is felt on touching the prime conductor of an electrical machine, when charged with the electrical fluid from the globe. Like that it chiefly affects the ends of the fingers and the elbows. When the tail of the fish is gently held by one hand, and the head touched with the other, a very violent shock is felt in both elbows, and through the breast and shoulders. Mr. Bryant, at first, imagined that the violence of the shock proceeded from both arms receiving it at the same time, and that the pain was no more than that of the two shocks added together: But in this he found himself mistaken; for when seven persons joined hands, if, when the first gently held the tail, the seventh touched the head, all were affected at the same instant, in both elbows, in the same manner as happens in electrical experiments."* The experiments of Mr. Bryant were almost sufficient to establish the identity of the electric fluid, and the medium through which these phenomena were produced. His commentator says, "From these observations, there can be no doubt, that the peculiar properties of this animal depend upon electricity."† This fact was considered to be proved by Mr. Walsh, and subsequently we find Galvani engaged in an experimental investigation of the same interesting subject. The result of the inquiry, instituted by this celebrated philosopher, has still further confirmed the conclusion which had been deduced from the experiments of Mr. Walsh. "From the earliest date of his discovery, he (Galvani) had paid a particular attention to the productions of Redi, Reaumur, Walsh, John Hunter, and Spallanzani, in which this important subject is investigated; and took advantage of the new data he had acquired on the subject of metallic irritation, to go still farther than his illustrious predecessors in this particular tract of science. In a voyage he made to the Adriatic Sea, he had a convenient opportunity to multiply his trials, which became the subject of a learned dissertation. His experiments being, however, more immediately connected with common electricity than galvanism, it will be sufficient in this place to observe, that the most interesting of them are those made by Galvani with prepared frogs. These frogs having been laid on different parts of the surface of torpedoes, and disposed in various directions, were all of them thrown into a convulsed state at the same moment. He obtained the same result from his subsequent experiments on the muscles and hearts of frogs placed on torpedoes; and established a new point of analogy, heretofore unknown, between the electricity of the torpedo and common electricity."* My only object is to point out the *analogy* which exists between the electricity of the torpedo and that of the atmosphere. The experiments of M. M. Humboldt and Gay Lussac, which are recorded in the hundredth volume of the *Annales de Chemie*, amongst many other important facts which they have established, have pointed out the *dissimilarity* between the two substances. They mention that the animal may be touched with impunity by means of a key, or any other metallic body, and that the most sensible electrometer, however applied, was not in the least affected by the organs of the fish. This fact may be satisfactorily explained by the supposition, (a supposition which I shall endeavour to shew is consistent both with reason and analogy,) that the electric fluid undergoes *certain changes*, and is modified by the actions of the living system, by which it acquires properties peculiar to itself, and becomes, in reality, not the electricity of the atmosphere but *animal* electricity. It may very reasonably be asked, whether this influence may not be wholly generated by the animal, and have no connection of any kind with the electricity of the atmosphere; but this idea is inconsistent with another fact recorded by these philosophers. They found that the animal, in a state of vigour, was capable of communicating the shock under water, but when exhausted, it lost this power; but if, in this exhausted state, it was raised above the surface of the water, it recovered the power of communicating the shock. We cannot suppose that the vigour of the animal was increased by removing it from its natural element; we must therefore imagine, that it acquired something from the atmosphere, with which the power of communicating the shock is immediately connected. This fact does not rest upon the authority of these gentlemen alone. In the Transactions of the American Philosophical Society, Mr. Flagg has enumerated some of the phenomena which this singular animal presented, and amongst others, he mentions, that it raises its head every five minutes above the water, as if to respire.

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* Med. Comment. 1787. † Ibidem.

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* Wilkinson on Galvanism.

It is generally supposed that the animal has the power of communicating the shock at will; and, indeed, without this power, the whole property is an useless appendage. When stunned by a blow upon the head, the animal may be touched with impunity; the shock, therefore, does not appear to arise from the general accumulation of this influence in the system. We must presume, that it is capable of being concentrated by the will of the animal; and there are strong reasons for supposing, that this power is intimately connected with its nervous system. Mr. John Hunter has accurately described the anatomical structure of the electrical organs of the Torpedo and Gymnotus Electricus; and by a comparative survey of these animals, and others of the same class, has thrown much light upon the nature of those actions, by which the peculiar effects we witness, are produced. "He has put up equal portions of the brain and medulla spinalis of the Raia Torpedo, and a more common ray fish of equal size, so that they appear like the front and back view of the same preparation, in order to shew their disproportion, and the *immense size of the nerves*, which go to the electrical organs."* Sir Everard Home has described not only the size, but the number of these nerves. "The voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and of defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for in these fish, the nerves connected with the electrical organ, exceed those that go to all other parts of the fish, in the proportion of *twenty to one*."*

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* Abernethy's Physiolog. Lect.

Can we, under all these circumstances, deny, that electricity must be an important agent in the animal economy? Is there not reason to suppose, that the quantum of this principle, which exists in the animal frame, is different at different times? Is it not more probable that the actions of the system should be regulated by its own inherent powers, than by the agency of substances foreign to it? and if so, is it not probable, that the electric fluid, in order to be converted to the purposes of the animal economy, must be previously changed and modified by the actions of the living system? Is there not a manifest connection between the phenomena which we attribute to the agency of this principle, and the development of the nervous system? Can any system of physiology be correct, which admits that electricity is a component part of the living organization; that the actions of the system are occasionally subservient to its influence; and, at the same time, by disregarding that influence, virtually denies what it is under the necessity of admitting?

We know that the human body is a conductor of electricity. If electricity answered no good purpose in the animal economy, is it probable that the latter would have been gifted with the power of receiving it so readily, when other substances are so constituted as to be capable of offering considerable resistance to its ingress? Can we, for instance, imagine, that less regard has been paid to the welfare of the human body, than to a piece of glass? If electricity be always useless, and often pernicious, can any good reason be given, why the body should have been made a conductor of this substance at all? We know that a stroke of lightning will often instantaneously destroy life. Can we suppose that the animal frame has been gifted with the power of conducting electricity, for the sole purpose of being injured by it? The magnitude of the injury occasionally derived from the passage of this fluid through the body, is, I conceive, a proof, a rational proof, of its vital importance to the animal frame at other times. In proportion as we admit that the occasional injury is great, so must we admit that the advantage derived from its constant influence is great also; otherwise we imagine, that the human body has been gifted with a property to its own detriment.

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* Philosoph. Transact. 1809.

If electricity be such an important agent in the animal economy, how, it may be asked, does it happen, that when an individual is overcharged with electricity, by being placed in contact with a highly electric body, and being at the same time insulated from others, which can convey it away, that so little constitutional effect is apparently produced? I shall endeavour, in a future part of these observations, to shew, that electricity does not, *of itself*, produce more effect upon animate than inanimate bodies, but that it must undergo a certain change, before it can influence the action of living matter; it must become *animal* electricity. It being admitted that the influence thus generated is distributed through the animal frame for the purpose of supporting its action, we must presume that the animal frame has the power of regulating the quantum of influence generated, otherwise we suppose, that the actions of the human body, are more dependent upon circumstances foreign to it, than upon its own organization. Electricity therefore, may be accumulated in the system, and may be the *source* of that influence which we have denominated animal electricity, and which the body has the *power* of generating; but unless that power be exerted, electricity is no more the influence upon which living action is dependent, than our food, before it has been acted upon by the stomach, is chyle. Electricity, thus undigested, (if I may use the expression,) has no more influence upon animate matter, than a living seed, which passes through the intestinal canal unchanged in its structure, has upon the organization of the system. We thus see why electricity may be accumulated in the system, and may be the source of living action, and yet, that this action shall not at all correspond with the quantum of electricity existing in the body, because electricity unchanged unmodified and undigested by the powers of the system, has nothing to do with its action.

The same argument applies to the objection, which may probably be made, that electricity will pass through a hundred bodies apparently unchanged, and according to the known laws of that matter, may be received from the hundredth body, in the same state as it was originally communicated by the electrical machine. It has not in reality undergone any change, and therefore its laws remain unaltered.

What *proof* have we, it may be asked, that this change ever takes place? This question must originate with a mind not very well acquainted with the nature of physiological inquiry in general. What *proof* have we that oxygen ever passes from a gaseous into a solid state? and yet the theory of animal heat, which is altogether founded upon the assumption that it does so, was not rejected upon the ground that this fact had never been *proved*. I might be justified in assuming the fact, if, by such an assumption, I could more satisfactorily explain the nature of living action. Dr. Paley says, "For instance, the principle of muscular motion, viz. upon what cause the swelling of the belly of the muscle, and consequent contraction of its tendons, either by an act of the will, or by involuntary irritation, depends, is wholly unknown to us. The substance employed, whether it be fluid, gaseous, elastic, electrical, or none of these, or nothing resembling these, is also unknown to us: of course the laws belonging to that substance, and which regulate its action, are unknown to us. The nervous influence, (for we are often obliged to give names to things which we know little about,) I say the nervous influence, by which the belly or middle of the muscle is swelled, is not mechanical. The utility of the effect we perceive; the means, or the preparation of means, by which it is produced, we do not: but *obscurity* as to the origin of muscular motion, brings no doubtfulness into our observations upon the sequel of the process."* But the fact, which I might be justified in assuming, is not wholly imaginary.

In the first place, arguments drawn from analogy are not wanting; and although such arguments are far from being conclusive, yet, they may be accepted in proof of the fact as far as they go: they give, at least, an additional probability to that, which is, in reality, assumed. I have already alluded to the points of similarity between the change, which the food is proved to undergo in the stomach, and the change, which electricity is imagined to undergo in other parts of the system. We all admit that oxygen is an important agent in the animal economy; without it the animal dies; and yet, the effects, produced by accumulating oxygen in the system, are comparatively insignificant. We may reconcile these facts, by supposing, that the human body has a limited power of assimilating oxygen to itself, and that this assimilation must take place, before any of the effects, which we attribute to the agency of this principle, can be witnessed. That oxygen, therefore, which is accumulated in the sytem, but remains unassimilated, has no influence upon living action. For this reason, if an animal be destroyed by the want of oxygen, that is, if the want of oxygen be the *sole* cause of its death, we cannot, by any supply of oxygen, re-animate the body; because that power, by which this influence can alone be converted to the uses of the animal economy, is irretrievably lost. In support of this fact, we have already stated, that the oxygen which exists in union with arterial blood, is different from that oxygen which exists in the atmosphere; it has, therefore, undergone some change, by which it ceases to be oxygen, and becomes another substance, which we have denominated the arterial influence; and yet, we cannot deny, that the oxygen of the air, and the arterial influence of the blood, have some intimate connection. We may infer, therefore, from analogy, that electricity may be accumulated in the system, may be an important agent in the animal economy, and yet, that the effects produced by this accumulation, may be comparatively insignificant; because that power, by which alone electricity can be rendered capable of influencing living action, is limited to a certain extent. This power cannot be regulated by the quantum of electricity existing in the body, because, under those circumstances, the actions of the system would be more dependent upon the state of the atmosphere, than upon its own organization.

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* Natural Theology.

But the fact does not rest wholly upon analogy. We have already seen, that the oxygen of the animal system differs essentially from that of the atmosphere: it in reality ceases to be oxygen. Have we any reason to suppose, that animal electricity differs from that of the atmosphere? does it cease to be electricity? The points of resemblance between the effects produced by the electricity of the atmosphere, and more particularly by galvanism, and those produced by animal electricity, are sufficient to justify us in assuming their connection at least; because, upon precisely the same grounds, we assume the fact, (and the fact I may observe is not questioned,) that a connection exists between the oxygen of the air and the arterial influence of the blood. It will not therefore follow, that, because animal electricity is different from that of the atmosphere, it has nothing to do with it.

There is reason to suppose, that animal electricity is conducted more readily by animal matter than by any other body. M. Humboldt, in his experiments upon galvanism (a species of electricity which seems to approach very nearly to that which is generated by the actions of the living system,) has arranged the conducting powers of different substances in the following order, beginning with those, which he considers the most efficient; metals, carbon, touchstone, muscular flesh; and yet, in the experiments made by M. M. Humboldt and Gay Lussac upon the torpedo, (which experiments are recorded in the hundredth volume of the *Annales de Chemie)*, we learn that the torpedo, when in a state of irritation, and readily communicating the shock to any individual who *handles* it, may be touched by the same individual with a *metallic rod*, and receive no shock. I have it not in my power to refer to the article which mentions this important fact; but I state it upon the authority of the following passage:—"The shock is obtained by a mere touch of the finger; but the contact must be immediate, for metals seem to be non-conductors of its powers; thus, the animal may be touched with impunity by means of a key, or any instrument of metal. The most sensible electrometer, however applied, was not in the least affected by the organs of the fish."* The observations of Mr. Flagg approach very near to the same point. "He found that the animal might be touched with a *metallic* rod enveloped in a silk handkerchief, with impunity."* Few persons, I presume, would venture to communicate with a powerful electric battery in the same manner. Dr. Fowler, in speaking of the galvanic influence, says, "This influence did not affect the most sensible electrometer; so far it agreed with the influence of the torpedo :—it passed readily through a metallic chain; this, however, opposes *an insurmountable* objection to the shock of the torpedo."† Water is a bad conductor of the former, but the latter cannot be communicated from one body to another, without the intervention of a fluid. "As electric fish, when healthy, exercise their powers as forcibly beneath the water as in the open air, the conducting properties of this fluid were examined. Several persons formed a chain of hands between the superior and inferior surfaces of the organs of the torpedo. The shock was not felt until they had *wetted* their hands. (This fact is wholly inapplicable to the electricity of the atmosphere.) The action was not interrupted when two persons supported the torpedo with their right hands, and instead of holding each other's left hand, they each plunged a metallic rod into water, placed upon an isolated body."* This last fact seems to be in opposition to that, by which it was inferred, that metallic bodies would not communicate the shock from the fish to an individual; how can these opposing facts be reconciled? Is it not probable, that the electricity conveyed by the metallic rods, was not, in reality, the animal electricity derived from the fish, but that electricity which existed in the *human* bodies in an unassimilated state? It is by no means ascertained, that the electric shock, which is experienced by many individuals, who communicate with an electrical machine through the medium of their several bodies, is derived from the passage of that individual electricity, which is, in the first instance, given out by the machine. Mr. Wilkinson says, "All fluids yet known, except air and oil, contain more or less electricity, and will freely admit its ingress as well as egress. As the human body is principally constituted of fluids, it is replete with electricity, and sensible of the least disturbance. A person insulated, giving a spark of electricity, communicates not identically the *same* portion he received from the machine, but an equal quantity, forced out of the body, by the impulse of that he received from the conductor."*

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* Retrospect of Discoveries.

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* Transactions of the American Philosophical Society.

† Med. Comment. 1793.

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* Retrospect of Discoveries.

These different facts, though apparently contradictory in themselves, may easily be reconciled, by our admitting, that the electricity of the human body, I mean that which has been assimilated with it, and which is, therefore, strictly speaking, *animal* electricity, is essentially different from that which exists in the atmosphere; but that the electricity of the atmosphere forms a component part of *animal* electricity, in the same manner as oxygen constitutes a component part of the arterial influence; and I have endeavoured to shew, that the idea is not wholly imaginary, (which, if proved, would not be an insuperable objection to my assuming the fact,) but that it is supported by arguments drawn from observation, analogy, and experiment.

But the idea is still farther supported by its rationality, by the *reasonableness* of the fact assumed. If animal electricity be such an important agent in the living system, and its laws be considered as precisely analagous with those of electricity in general, we could never come into contact with any body electrified minus, without parting with the very principle of our existence.* It has been supposed, with some reason, that the coverings of animals, such as hair, wool, and feathers, are intended to preserve the temperature of their bodies, and they are imagined to produce this effect in consequence of their being bad conductors of heat: but are they not equally bad conductors of electricity? The oil-bag is used by birds, for the purpose of anointing the surface of their bodies, and the object is considered to be that of throwing off the wet and moisture, which would otherwise be in contact with them; but the oil bag is not confined to aquatic animals. The canary bird in his cage has recourse to the oil-bag as frequently as the duck in the pond. It is supposed to be used for the purpose of cleansing the feathers. But man has also innumerable oil-bags communicating with the surface of his body. "We see the ducts of the oil-bags terminating on the surface of the cuticle. It seems here right to observe, that myriads of oil-bags are disseminated throughout the skin, from which ducts proceed to the surface of the cuticle to anoint it every where; and that it is most liberally oiled in parts subject to the greatest friction or evaporation."* In birds, then, the oil is supposed to answer the purpose of throwing off moisture from the surface of their bodies, or to assist in cleansing their feathers; but in man it answers neither of these purposes: it is intended to prevent friction and evaporation. In fishes, it is supposed to answer another purpose, that of enabling them to slide readily through their natural element. "In evidence of this, I may mention, that the surfaces of fish are smeared over with mucus, which is liberally poured out from the orifices of numerous ducts, rendering the surfaces of these animals particularly slippery, preventing also any adhesion between them and the element in which they move, and serving the same purpose in facilitating their transit through the water, that greasing the bottom of a boat is known to do."† Amongst these various uses which have been attributed to the natural oil of animals, there is one which has hitherto been neglected, but which appears to be applicable, without exception, to all animated beings. Mr. Wilkinson informs us, in the passage already quoted, that oil is absolutely a non-conductor of electricity. May we not therefore

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* Med. & Phys. Journ.

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* I do not imagine that animal electricity and life are the same things; but that the former is essential to the existence of living *action*.

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[*] Abernethy's Physiolog. Lect.

[†] Ibidem.

presume, that the natural oil is intended to prevent the unnecessary expenditure of this principle, and that it answers this individual purpose in all animals? Dr. Paley says, "every feather is a mechanical wonder. If we look at the quill, we find properties not easily brought together—*strength* and *lightness.* I know few things more remarkable, than the strength and lightness of the very pen with which I am writing. If we cast our eye to the upper part of the stem, we see a material, made for the purpose, used in no other class of animals, and in no other part of birds; tough, light, pliant, elastic. The *pith*, also, which feeds the feathers, is amongst animal substances, *sui generis;* neither bone, flesh, membrane, nor tendon."* It is difficult to imagine, in what manner this peculiar substance can add either to the strength or lightness of a feather; but every electrician is acquainted with the properties of a pith-ball, and will readily admit, that this peculiar substance may answer an important purpose, by being a non-conductor of electricity. If the hair of animals be solely intended to preserve the temperature of their bodies, why should it abound in the mane of the lion? The other parts of his body are more remote from the source of circulation, and are consequently more liable to those inconveniencies which arise from too rapid an expenditure of animal heat; they ought, consistently with this idea, to be more clothed than the neck. The wonderful strength of this animal is admitted to be more connected with the muscles of the neck, than with those of any other part of his body. If the action of an organ be dependent upon a certain portion of animal electricity existing in it, we can at once see why the rapid expenditure of this influence should be particularly guarded against by a provision of nature for the purpose. Other instances of this kind might easily be adduced, but I do not consider it as an argument of much weight; neither do I wish it to be understood, that the coverings of animals may not answer other purposes than that which I have suggested. Considering the subject in a general point of view, we have great reason to suppose that electricity is an important agent in the animal economy, if we find that nature has, in a peculiar manner, guarded against its rapid and unnecessary expenditure.

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* Natural Theology.

We all *feel* that, to a certain extent, our health is influenced by the state of the atmosphere? and some persons, from their sensations alone, will foretell a thunder storm.

How can that system of physiology be correct, which does not even attempt to explain a fact which it is impossible to deny ?

It is probable that I may have attached too much importance to the agency of animal electricity; but with such positive evidence of its occasional influence, and with so many corroborating proofs of the effects, which it is capable of producing in the living system, I cannot agree with the great mass of physiologists who admit these facts, and yet at the same time disregard them. I cannot persuade myself, that the animal frame has been made a conductor of electricity, for the sole purpose of being injured by it ; and yet what other conclusion can be drawn from the writings of those physiologists, who consider electricity to be a substance, wholly inert as far as the *healthy* actions of the system are concerned, but capable, at the same time, of *destroying* those actions altogether. I can have no hesitation in saying, that these physiologists must be wrong. No apology, therefore, can be necessary, on my part, for having deviated from a system, which appears to be so inconsistent with reason and common sense ; and I have only to request, that those authors, who object to the theory which I have presumed to offer to the profession at large, will not do so, without, at least, some attempt to offer a better; but I have no hesitation in repeating, that the theory, which disregards the agency of electricity altogether, must be wrong.

We are not sufficiently acquainted with the laws of animal electricity, to be enabled to prove that it unites with and becomes a component part of the living organization. We cannot, for instance, prove, that animal electricity and the nervous influence are the same things, but I shall have occasion, in a future part of these observations, to suggest the probability that they are so. Those physiologists who admit that living action is dependent on the nervous influence, must also admit that it unites with the organization of the system; because the effects attributed to this influence, continue for a certain time after the supply has been intercepted. Thus, if the action of the heart be dependent upon the nervous influence, and that action be capable of being excited, after the heart has been removed from its connection with the body, we must either admit that the influence continues to reside in that organ, or that the action of the heart may be excited without it. Analogy, therefore, would lead us to conclude that electricity is in reality assimilated with the body.

I shall not, at present, attempt to trace the electricity of the atmosphere into its union with living animal matter, because the subject involves many points which have not yet been discussed; but an important question suggests itself, which cannot, with propriety, remain unanswered. Water is a bad conductor of electricity; whence do fish derive this influence? Although water be admitted to be a bad conductor of electricity, there seems to be no doubt that water contains electricity in its composition. The late discoveries in galvanism have placed us under the necessity of supposing, that all chemical action depends upon the quantum of electricity contained in the several particles of matter. "The means by which bodies are rendered electric, are so various, that it may be asserted, perhaps, without exception, that every change, whether mechanical or chemical, which can take place in them, will at the same time produce electricity."*

"It has been suggested by Mr. Davy, as a correction of the theory of Volta, that the electro-motion, occasioned by the contact of metals, is the primary cause of the chemical changes, and that these changes are in no other way efficient, than as they restore the electric equilibrium. To explain this, let us suppose, that in any three pairs of plates of the galvanic trough, the zinc plates are in the state of positive, and the copper plates in that of negative electricity ; the liquid, in any cell after the first, will be in contact, on the one side, with positively electrified zinc, and on the other with negatively electrified copper; and if the elements composing the fluid be themselves in *different states of electricity*, the negatively electrified element will be attracted by the zinc, and the positively electrified element by the copper."* It seems therefore, that fish are surrounded by an element which contains electricity. But the most apparent source of electricity, is the oxygen of the air, which is conveyed to the branchiæ in the act of respiration. Without oxygen we know that fish cannot live. All matter contains electricity, and the effect of oxygen in supporting combustion, would lead us to suppose, that this principle may contain electricity in a greater proportion than other substances. We have no reason, therefore, to suppose, that the finny tribe is cut off, by the intervening element, from the electricity of the atmosphere. The fact that the torpedo, in which the electric power is so manifest, often rises to the surface of the water for the purpose of respiration, and that when exhausted and incapable of communicating the shock under water, it regains the power by being removed from that element and brought into contact with the air, seems to point out, in a forcible manner, the connection between this power of the animal and the influence of the atmosphere.

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* Nicholson's Encyclopedia.

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* Henry's Chemistry.

I shall conclude this division of my subject, by observing, that the doctrine which I have espoused, is supported by the opinions and observations of some of our most able physiologists. It may indeed be considered as an endeavour to enlarge upon those views, which the discoveries of Galvani first opened, which were obscured by the intemperate zeal of his immediate successors, but which have again burst forth with additional splendour, illuminating every object connected with physiology, and penetrating into the inmost recesses of pathological research. In support of this observation, I may mention *pour fair la distinction*, the names of Darwin, Abernethy, Sir Everard Home, Dr. Wilson Philip, and Mr. Brande, and also quote the opinion so recently expressed by Mr. Swan, in his dissertation upon the treatment of nerves. "From repeated observations we ascertain the effects of the nervous influence, but of its nature, and of the manner in which it acts, we have little or no knowledge. Of all the theories that have been invented, none certainly appears so rational as that of electricity; and it seems the more plausible from this circumstance, that the galvanic influence on the nerves of an animal nearly dead, will produce the same motions in the parts to which these nerves are distributed, that were produced in them when the animal was alive. The fact of the Torpedo and Gymnotus Electricus being able to give shocks exactly like those produced by electricity, and their being provided with a particular apparatus, which is supplied with large nerves entirely for this particular purpose, leads still further to the presumption, that the nervous influence is very much like electricity, if not the same thing." I have only two observations to make upon this passage: 1st, The theory receives additional confirmation from the circumstance, that the peculiar effect of living action, appears to be that of counteracting all the most remarkable chemical affinities; and recent discoveries have pointed out the same effects as the results of galvanic action. 2nd, There seems to be a good reason why the nervous influence should rather be a peculiar modification of electricity, than electricity itself; because, supposing it to be the latter, and to be amenable to the known laws of electric matter in general, the actions of the system would necessarily be more dependent upon circumstances foreign to the body, than upon its own organization.—I shall proceed to the consideration of the nervous influence, which we suppose to be a peculiar modification of electric matter.

CHAPTER XI.

ORGANIZATION—NERVOUS INFLUENCE.

WE have already traced the oxygen of the air to its union with the blood, and have suggested the probability, that it combines with some other principle; that by this combination its original properties are lost; and that the result of this combination, is what has been denominated the arterial influence. I shall now briefly state, that the arterial influence is not oxygen alone; and this fact I consider to be proved by the circumstance, that the characteristic properties of oxygen are well known, are easily detected by chemical tests, but cannot be discovered in the blood. This objection is far from being obviated by the ingenious suggestion of Dr. Bostock, that oxygen unites with the blood in a solid, and not in a gaseous form, because oxygen in a solid form is altogether an imaginary substance, the laws and properties of which we are wholly unacquainted with. Mr. Ellis, in his reply to this author, says, "Dr. Bostock very truly observes, that many parts of my reasoning are founded upon the mistaken opinion, that air is supposed to enter the blood, and to exist in the vessels; while, according to his hypothesis, it is not *oxygenous gas*, but oxygen which is supposed to mix with the blood. I cannot, however, help thinking, that, if the arguments I have adduced against the entrance of gaseous fluids, have any weight, they bear, with at least equal force, against the idea, that the *ponderable* parts of the gases are able to pass through the cells and blood vessels of the lungs. We know *nothing* of the properties of oxygen, but as we see it in combination with some other substance, and surely of all combinations, *its union with caloric* would appear to be that which gives it the greatest power of penetration. When therefore, it is made to lose its caloric in the bronchial cells, and to become an inelastic body, the difficulty of supposing it to enter into the blood, is, in my conception, greatly increased."*

If we have no hesitation in admitting, that oxygen exists in a gaseous state, in consequence of its union with caloric, it can hardly be considered as assuming a fact, when

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* Med. & Surg. Journ.

I say that oxygen is capable of uniting with electricity; because it is by no means clear and generally admitted, that caloric and electricity are not the same things; but at all events, the laws and properties of each are so similar, that it is almost impossible to grant the union of one, and to deny that of the other. Instead, therefore, of saying, that oxygen, previous to its union with the blood, passes from a gaseous into a solid state, and gives out its caloric, I should say that it unites with electricity, and that the result of this union is the arterial influence.

Whence, it may be asked, is the electricity derived? The atmosphere is a compound substance consisting of nitrogen, oxygen, carbonic acid, and aqueous vapour, and these substances appear to exist in the following proportions: nitrogen gas, 75.55; oxygen gas, 23.32; carbonic acid, 0.10; aqueous vapour, 1.03; so that almost 99 parts in 100 are nitrogen and oxygen, and these exist in the proportion of more than 3 parts of the former to 1 of the latter. Upon the surface of the globe these proportions seldom if ever vary; that is, if we except the aqueous vapour. This exception has led to the opinion, that the aqueous vapour does not enter into the *composition* of the atmosphere, but that it is only mechanically united with it; so that the atmosphere, strictly speaking, is a compound body, consisting of nitrogen, oxygen, and carbonic acid. It is of consequence to my argument, that this distinction between the mixture and the combination of the several elements contained in the air, should be established. Dr. Henry says, the air of our atmosphere, it appears from the foregoing facts, is a *mixture*, or possibly a *combination* of two different gases, viz. oxygen gas and azotic gas. But it is in my opinion demonstrably proved, that a chemical action is going on in the air. We find that the atmosphere does not contain more than 10 parts in a thousand of carbonic acid, and yet, in the expiration of each individual, " atmospheric air, after being once only admitted into the lungs, returns charged with 8 or 8½ *per cent.* of carbonic acid gas." What becomes of this immense, this incalculable quantity of carbonic acid gas, which is returned to the atmosphere ? It cannot possibly be *mechanically* united with the air; it must undergo some sudden change, by which its characteristic properties are lost. The uniform *proportion* of the several elements to each other is absolutely inconsistent with the idea, that these elements are mechanically united. I conclude, therefore, that the atmosphere is a *compound* body, consisting of oxygen, nitrogen, and carbonic acid.

Meteorology is that science which attempts to explain the phenomena of the atmosphere, and is wholly founded upon the doctrine, that " constant changes are taking place, the consequences of new combinations and decompositions rapidly following each other;"* and the same author says, "The means by which bodies are rendered electric are so various, that it may be asserted, perhaps, without exception, that *every* change, whether mechanical or chemical, which *can* take place in them, will at the same time produce electricity. or *alter their electric states*." The discoveries of Sir Humphry Davy have, in a remarkable manner, confirmed this doctrine; and the chemical theory now established, is this; that the particles of matter, in order to be capable of acting upon each other, must be in different states of electricity, and that the action itself is, in reality, the attempt to restore the electric equilibrium. In the decomposition, therefore, of a body, the electric equilibrium, by which the particles of that body are kept in chemical union, must be disturbed, before any decomposition

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[*] Encyclopedia.

can be effected. We know that the air is decomposed in the lungs; we know, therefore, that the equilibrium of electricity, which kept the several particles of oxygen, nitrogen, and carbonic acid, in that state of union which constituted the atmosphere, has been disturbed. Why may not the oxygen, in its free and uncombined state, have acquired electricity from the other elements? if so, that union of oxygen with electricity, which we have denominated the arterial influence, is at once accounted for. But there is reason to suppose, that this change has actually taken place. However we may question the identity of electricity and caloric, it is, I conceive, impossible to deny, that they are, at least, modifications of each other. Oxygen, the element which has been lost in respiration, supports combustion in a remarkable manner; nitrogen, and carbonic acid, the elements which have been separated from the atmosphere, and are returned to it in the act of expiration, extinguish combustion. This fact admits of an easy explanation: oxygen has a greater capacity for electricity than either nitrogen or carbonic acid. The former, in its union with an inflammable body, passes from a gaseous into a solid state, by which its capacity for electricity is diminished, and that principle, in consequence, evolved; the latter, in their union with an inflammable body, also pass into a solid state, by which their capacity for electricity is increased; this principle, therefore, is taken from, instead of being added to, the inflammable body, and combustion is extinguished, instead of being supported, by them. The analogy between caloric and electricity, fully appears to justify this argument; and if we admit, without hesitation, that oxygen, in passing from a gaseous to a solid state, gives out caloric, it will, I think, be extremely difficult to prove, that it does not give out electric matter.*

Is this decomposition of the air to be attributed to any action of the lungs themselves, or to the influence of the blood? I shall defer the consideration of this subject, until I inquire into the nature of respiration, regarded as a living action, by which the effects we witness, are produced.

Is the arterial influence distributed to the several parts of the system, through the medium of arterial blood alone? I shall come to the point I wish to establish, more directly, by putting the question in a more tangible form. Is the arterial influence distributed to the several parts of the system, through the medium of the *nerves*. Mr. Burns says, the vitality furnished by the *air*, seems to be yielded to the *nerves* during the course of circulation, for the immediate performance of the actions of the system." In corroboration of the theory I have advanced, he also says, "This oxygen changes its state in the blood; for I know of no experiment which proves that it is found in a gaseous state in the blood."*

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* Mr. Abernethy says, "But if science were *eventually* to demonstrate that heat, light, and electricity, are different things."

Dr. Darwin says, "The similarity of the texture of the brain to that of the pancreas, and some other glands of the body, has induced the inquirers into this subject to believe, that a fluid, perhaps much more subtile than the electric aura, is *separated* from the blood by that organ for the purpose of sensation and motion. When we recollect, that the electric fluid itself is actually *accumulated* and given out voluntarily by the Torpedo and Gymnotus Electricus, that an electric shock will frequently stimulate into motion a paralytic limb, and lastly, that it needs no perceptible tubes to convey it, this opinion seems not without probability; and the singular figure of the brain and nervous system seems well adapted to distribute it over every part of the body."† Mr. Le Gallois says, "La vie est due à une impression du sang arteriel sur le cerveau et la moëlle epinìere, ou a une principe resultant de cette impression."*

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* On Inflammation. † Zoonomia.

By one author the vitality of the *air* is supposed to be distributed through the medium of the nerves; by another, a peculiar substance is imagined to be *separated* from the *blood;* and by another, this peculiar substance is supposed to be *generated* by the brain, the blood simply acting as a stimulus to that organ, by the action of which this peculiar substance is generated. The usual phenomena of fainting are sufficient proofs of the connection between the supply of *arterial* blood, and the action of the brain, and M. Bichât has established the fact by actual experiment. Is then the arterial influence communicated to the brain unchanged, and in the same state as it exists in arterial blood? Is it communicated to the brain in a different state, having been changed by the action of the blood vessels? Is it communicated, in either of these states to the brain, simply exciting its action, by which a *new* substance is generated? or, does it simply act as a stimulus to the brain, enabling that organ to communicate to the several parts of the system, a substance accumulated in it, and derived from some other source than the blood? The latter idea is wholly imaginary, is unsupported, not only by observation and experiment, but even by analogy; and would be a glaring exception to all the laws of living animal matter, with which we are at present acquainted. It is clear that Dr. Darwin imagined the nervous influence to be *separated* from the blood by the *secretory* action of the brain, because he has alluded to the resemblance between that organ, and the pancreas which is generally admitted to be a secreting gland; and Mr. Le Gallois cannot be supposed to have entertained any other idea, because it naturally arises from what he has said, and is not otherwise expressed. Mr. Burns, on the contrary, says, that the vitality of the *air* is communicated to the brain, and distributed through the medium of the nerves; which seems to imply, that the air has *not* been changed by the action of that organ. These opposite opinions necessarily involve the question, whether the brain is, or is not, a secreting organ.

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* Experiences sur le Principe de la Vie."

There is one circumstance which forcibly suggests the idea, that the brain is, in reality, a gland. I allude to the quantity of blood which is sent to that organ, and which must be admitted to be more than we can imagine to be necessary to the support of its structure alone. But the same thing applies to the heart; and yet no one imagines the heart to be a secreting organ. The heart receives more blood than is necessary to the support of its own individual structure, for the purpose of distributing it to the several parts of the system. Why may not the brain receive a greater portion of the arterial influence, than is necessary to its own individual action, for the purpose of conveying it to the several parts of the system?

If we imagine that the brain is a gland, we must also suppose that the nerves are its excretory ducts. Now the only reasons for denominating an organ a gland are these: we see that a quantity of blood, greater than we can imagine to be necessary to the support of its structure or action, is conveyed to it; we see that a substance, differing from the blood itself, is separated from that fluid by the action of that organ which we term a gland; and the substance secreted is collected to a given point, and conveyed through the medium of a canal, (denominated a duct,) to some other part of the system. I will readily admit, that the substance secreted may exist, although it cannot be recognised by our weak and imperfect senses; and that an aërial substance may be secreted, although we are unable to discover it. I will readily admit, that a gland may be *imagined* to have a thousand ducts, without losing its claim to be regarded as a secreting organ; and I would even admit, than an organ may be a gland without any duct at all, provided any other mode is apparent, by which the substance secreted is conveyed from it. I do not object, therefore, to the brain being considered as a gland, because the substance secreted is invisible, because its ducts are numerous, or because it has no ducts at all. But if we once admit, that the action of an organ does not correspond with its structure, physiology is no longer a science, anatomy is worse than useless, the theory of medicine a vague and wild hypothesis, and its practice a system of chicanery and deception. I cannot for a moment suppose, that a gland, which performs the act of secretion, and its duct, which has nothing to do with that act, but merely conveys the substance which has been secreted, can, under any circumstances, be precisely similar in structure.

Those physiologists, who attribute to the brain a glandular action, imagine the nerves to be simply ducts, whose peculiar office it is to convey the substance secreted, whatever it may be, from the brain to the several parts of the system. But the structure of nerve and of brain is precisely the same; how then can we imagine the function of each to be so different as that of a secreting organ and of its duct? This similarity of structure is acknowledged by the highest authorities. Baron Haller says, "All the medulla of the brain and cerebellum goes out from the skull through particular openings, towards certain parts to which it is directed. The smaller bundles *of this medulla* we call nerves."* Dr. Monro says, "The nervous cords, thus composed of nervous fibrils, .cellular coat, *pia and dura mater*, have such numerous blood vessels, that after their arteries only are injected, the whole cord is tinged of the colour of the injected liquor." How exactly does this description agree with the anatomical structure of the brain; we suppose the brain to be a secreting gland, upon the sole ground of its vascularity, and yet nerves, "having such numerous blood vessels, that after their arteries only are injected, the whole cord is tinged of the colour of the injected liquor," are imagined to be almost passive organs, having no other function to perform, than simply to convey the substance secreted in the brain to the several parts of the system. Dr. Darwin says, " A nerve is a *continuation* of the medullary substance of the brain from the head or spine towards the other parts of the body, wrapped in its proper membranes."* Mr. Burns says, " The brain is a soft globular substance contained within the cranium, of a dusky colour without, but white in the centre. It is from this white or medullary matter, that we find all those elongations arising, which are called nerves, and which are *portions of the brain*, diffused over the body, and exerting there the peculiar properties of the system to which they belong."† The inference, which must necessarily be drawn from these facts, is this, that the structure of nerve and brain being the same, their function must also be the same. Do we in reality find this to be the fact ? The strongest proof of this fact is founded upon the circumstance, that the function of nerve continues after the brain has been destroyed. In acephalous monsters, we observe no deficiency of nerves, although there is no brain from which they can emanate. Mr. Spurzheim says, " Sometimes the head, the upper parts of the body, and the thorax are wanting, and yet the inferior parts have nerves. Haller, Soemmering, and many others, speak of such monsters, consisting only of the lower half of the body, from the umbilical cord downwards. The last, which I examined not long ago, is in the possession of Mr. Norman, of Bath. All its parts are tolerably perfect. There is in it a corresponding portion of spinal marrow, rounded at the upper extremity, and inclosed in dura mater. The lower limbs contain the *usual nerves.* *Even a leg*, if it be born alone, is provided with them; hence the brain *cannot* be their origin."*

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* Physiology.

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* Zoonomia. † On inflammation.

We have positive evidence, that a fœtus in utero may live without a brain, but the following is, I think, one of the most satisfactory instances of this kind of malformation. "In this curious case of monstrosity, the bones of the cranium were wanting; there was not any brain, or the least portion of medulla oblongata; a ragged membranous open pouch, coming from the scalp, hung down on the back; the features, extremities, and parts of generation were *large* in proportion to the size of the monster. Organization, then, is not dependent either upon the brain or spinal marrow."† We have here the singular anomalies of the ducts of a gland existing without the gland itself, and the former performing the function of the latter. Did we ever hear of the ductus hepaticus secreting bile, or of the ureters elaborating urine? Certain parts of the body were *large* in proportion to the size of the monster. If the brain has any thing to do with the organization of the system, the nerves must, in this instance, have performed its function. There is every reason to suppose, that, in the fœtus, the nerves are formed *before* the brain—the ducts before the gland! Dr. Spurzheim says, " The doctrine of a central point, and of a single origin of all the nerves, is neither true nor possible, and this may be verified by examination."* Upon the whole, the only rational view of the subject appears to be this:—That the brain is not confined to the cranium, but is distributed through the body; assuming, in the cranium, that peculiar form which we term cerebrum, cerebellum, and medulla oblongata; in the spinal canal, that peculiar form which we denominate medulla spinalis, and in the nerves, the peculiar organization of these organs.

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* Physiognomical System.

† Cam's History of an extraordinary Fœtus.

The doctrine of a nervous fluid, imagined to be secreted in the brain, and conveyed from thence, through the medium of the nerves, to every part of the system, has been so long and so universally adopted, that it is difficult to suppose, that the opinion is altogether erroneous. Our predecessors, though wild and unrestrained in their theories, were accurate observers of facts. We have a strong proof of this, in their theory of the nervous influence. They had not failed to observe the remarkable connection which exists between respiration and living action generally, and between respiration and the function of the brain particularly. "The ancients believed this energy to be of a gaseous nature, and maintained that it was formed in the ventricles of the brain from the *air* which we inspired."* But they had some difficulty in conceiving how it got there, and could find no other channel than through the cribriform lamella of the ethmoid bone. Modern physiologists have no hesitation in admitting, that the blood is the source from whence the nervous energy is derived, although they differ materially as to the manner in which it is generated. I have already quoted the opinions of Mr. Burns, Dr. Darwin, and M. Le Gallois, upon this subject; but I shall defer the expression of my own sentiments, at present, because we must have some idea of the nature of this substance, before we can hazard any conjecture as to the mode in which it is formed.

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* Physiognomical System.

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* Galen de Utilitate Respirationis

The nervous fluid, or the nervous influence, (for it is of no consequence by what name we designate it) is wholly an imaginary substance ; that is, imaginary, not as to its existence, but as to its form, properties, &c. We have no idea of the nature of that substance called caloric, and yet we entertain no doubts as to its existence. We do not, in reality, know the *nature* of any one thing, with which we imagine ourselves to be acquainted. A piece of wood has certain properties, such as figure, density, weight, &c. But knowing these properties, we are no more acquainted with the nature of wood than we were before. It may consist of certain elements originally derived from the earth, and disposed according to a peculiar arrangement; but, of the nature of these elements we are wholly ignorant. All substances are derived from certain elements, and the word implies ignorance as to their composition. They are called elements, because they are the principia, the first causes of what we seem to know; but, if we attempt to advance one step further, we feel that there is a first cause of all, which we can never hope to know. Our knowledge, then, simply amounts to this;

that certain substances are endowed with certain properties. The acknowledged property of the nervous influence is to affect living action. It may be an element which we are incapable of decomposing; or the phenomena of that action which is dependent upon it, may lead us to suppose, that it is not an element *sui generis*, but some other, with the properties of which we are better acquainted; or it may be a compound of more elements than one, the properties of which are better known to us. Having ascertained this fact, we seem to have acquired a certain degree of knowledge; but we are, in reality, as far from being acquainted with the nature of the nervous influence, as we were before. If, for instance, electricity and the nervous influence were proved to be the same things, we should still be ignorant of its nature, because electricity is an element, the nature of which we are unacquainted with. If the nervous influence were proved to be a combination of oxygen with electricity, we should have acquired a certain degree of knowledge, but we should have no more idea of its nature than we had before, because oxygen and electricity are elements which we are incapable of decomposing. Our knowledge, then, simply amounts to an acquaintance with the properties of bodies, and with the elements of which they may be composed. The property of the nervous fluid is to influence living action; our object, then, is to ascertain in what mode this influence may produce the effects we witness, and of what elements it may be composed. If we admit it to be an element *sui generis*, that is, different from every other with the properties of which we are acquainted, we ascertain our *want* of knowledge, and thus every step in knowledge brings us nearer to ignorance, because it brings us nearer to the first cause of all, of which we can form no conception. But these observations apply only to an inquiry into the *nature* of a substance, and to an objection which might possibly be made, that being unacquainted with the tangible and visible properties of the nervous influence, no advantage can be derived from any attempt to ascertain its other properties; or that being unacquainted with the nature of the elements of which it may be composed, it is but lost labour to endeavour to ascertain, what those elements are. But if we are better acquainted with the properties of these elements, than with those of the nervous influence, it is clearly an acquisition of knowledge to ascertain that the nervous influence is composed of those elements.

with whose laws and properties we are more familiar. Electricity, for instance, presents phenomena which lead us to conclude, that it contains the principle of caloric; if the nervous influence contain electricity, we conclude that it also contains the principle of caloric, and thus an important fact is ascertained, which may lead to the explanation of animal heat. It is of no consequence to our argument, whether we are, or are not, acquainted with the *nature* of caloric; we know that there are certain effects which can only be attributed to the agency of that principle. "If we perceive an useful end, and means adapted to that end, we perceive enough for our conclusion. If these things be clear, no matter what is obscure; the argument is finished."

One property of the nervous fluid, is that of influencing living action; one property of the arterial influence, is that of influencing living action. When we observe an identity of effect, we naturally look for an identity of cause. We have already given our reasons for supposing that the arterial influence is a peculiar modification of electricity, that is, a peculiar combination of that element with oxygen. Is the nervous influence a peculiar combination of electricity with oxygen? We have already shewn, that Mr. Burns imagines the vitality of the air (that is, we presume, its oxygen, because it is the only aërial principle which is supposed to influence living action) to be conveyed to the several parts of the system, through the medium of its nerves; or he may possibly allude to the electricity of the air. If the word is intended to be accepted literally, that is, if he supposes the *vital principle* to be floating in the atmosphere, it is assuming a fact to no good purpose. It is calling upon us to grant certain premises, not for the purpose of explaining a fact, but for that of rendering it more obscure. It is also assuming a fact, which necessarily involves another. That portion of the air, which is found to be in a peculiar manner essential to the existence of the vital principle in animals, is its oxygen. Without this principle the animal dies. The vital principle, then, and oxygen, must either be imagined to be the same things, or there must be an intimate connection between them; so that when the author speaks of the vital principle circulating through the nerves, we must (assuming their identity) suppose that he means oxygen; or, if he does not mean oxygen, the author is called upon not only to tell us what he does mean, but to explain the cause of this intimate connection between the two, and to disprove their identity.

Irritability is that property of a muscle, which gives it the power of being excited into action. If therefore we point out the connection between this property of muscular fibre and the supply of oxygen, we shall at the same time point out the connection between the supply of oxygen and living action; and if it shall appear that the nervous influence contributes to the support of living action, not, as is generally imagined, by stimulating an organ, but by supporting its irritability, that is, its *capability* of action, we may fairly assume, that if this property of muscular fibre is owing to oxygen in one instance, it may with justice be attributed to that principle in another; and therefore, the nervous influence producing, in this respect, the same effects as oxygen, may be presumed to contain it. I shall for the present assume the fact, that the nervous influence *never* acts as a stimulus to an organ, but that it *always* acts by supporting its irritability, that is, its susceptibility of action only. This subject will be more fully discussed, when we consider the nature of living action in general, and more particularly the phenomena attendant upon muscular motion.

The connection between the supply of oxygen and the irritability of muscular fibre, was suggested by the experiments of Drs. Priestley and Mayow. Dr. Fothergill, in his Dissertation on suspended Animation, says, "From the observations of Drs. Mayow and Priestley, on vital air, and its effects on the blood and respiration, I was led to consider it as the *cause of irritability*, the principal agent in the animal economy; and therefore ventured, at an early period, to throw out some hints, towards expediting its application to medicine, and more particularly to the theory of animation. The idea, at length, being adopted by others, has of late been offered as entirely new." These hints are here supposed to have been adopted by Dr. Girtanner, but the merits of this discovery are generally attributed to the latter author. This theory was, at its first introduction, violently opposed, and the supporters of it were held up to public ridicule, under the title of the "*oxygenous philosophers*;" but it has passed through the fiery ordeal of public opinion; and instead of being *oxidized* in its passage, so as to lose its lustre, it continues to present a luminous surface, emitting rays of light, which not only penetrate into the dark recesses of physiological research, but throw light upon some equally obscure points of pathology. I have said, that this doctrine has passed through the fiery ordeal of public opinion, and so far from being deprived of its lustre, has emerged from it with additional splendour. In proof of this fact I shall quote the following authorities:—"It was long supposed, that the cessation of respiration occasioned that of the heart's motion, in consequence of the black blood not having sufficient power to stimulate its fibres; but does not the right side of the heart, which, under all circumstances, contains de-oxygenated blood, contract with a vigour equal to that of the left? It was reserved for Bichât to offer a true explanation of this phenomenon; he has very justly stated that, in consequence of the suspension of the respiratory function, the coronary vessels, by which the muscular structure of the heart is supplied, are compelled to carry black, instead of scarlet blood; a fact which, in itself, is quite adequate to explain the cause of the heart ceasing to contract; for the *irritability* of this, like that of every other muscle, can be alone maintained by duly *oxygenized* blood."*

"In death, from suffocation, the heart continues to pulsate for several minutes after the breathing has entirely ceased, in consequence of which, the blood which passes through the pulmonary vessels, no longer receives the *influence of oxygen*, and therefore black blood circulates; the brain, it would appear, soon feels the *want* of the *florid arterial* stream, by which *alone* its energies can be maintained."* M. Bichât found, that when dark-coloured blood was injected into the vessels of the brain, its functions were suspended, and that they were restored by the supply of florid arterial blood.

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* Paris' Med. Jurisprudence.

M. Cuvier says, " Modern experiments have *demonstrated*, that one of the principal uses of respiration is to re-animate the muscular force, by restoring to the fibre its exhausted *irritability*."†

I quote the following passage, because it not only points out the importance of oxygen to the system, but at the same time corroborates the fact which I have already stated, that the oxygen undergoes some peculiar change before it constitutes the arterial influence:—" In hanging or drowning, death is produced by the want of *arterial blood*; and the universal method of cure has been, to attempt the restoration of respiration, although this has not always been done upon the true principle. By inflating the lungs, respiration is often restored, but it also very frequently fails;

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* Paris' Med. Jurisprudence.

† Comparative Anatomy.

because the mere blowing in of air neither infallibly excites the action of the lungs, nor does it, as some suppose, change the blood; for this change is an action dependent on life, and cannot be imitated by the chemist more than *digestion*."*

It is impossible to deny, that a most intimate connection exists between respiration and the continuance of living action; and there seems to be no doubt that respiration contributes to the support of living action, by supplying oxygen. If, for instance, an animal be included in a certain portion of air, it dies in a short time; and if the air be then analyzed, we find that it has been deprived of its oxygen. If, on the other hand, before the life of the animal be extinct, we admit a certain portion of oxygen, the animal immediately recovers, and appears to have suffered no material injury. The same thing applies to the human frame. Innumerable examples to this effect might be recorded, but I select the following as one of the most remarkable and most interesting of the kind:—"It was in the month of June, 1756, that the Viceroy of Bengal laid siege to Fort William, (the English Factory at Calcutta). Mr. Holwell, assisted by the factors and the garrison, defended this post with extreme bravery, but was at length obliged to surrender. There were at this time remaining in the fort an hundred and forty-five men and one woman. The whole of this unfortunate company, many of whom were wounded, and several very dangerously, were shut up the same night in a small prison, only eighteen feet square. This prison, which is now better known in England by the name of the *black hole*, was inclosed by strong walls, and had only two small windows at one end, secured by iron grates. In this confined situation which allowed only a space of about eighteen square inches to each individual, the heat and *want of fresh air* soon excited the most horrible effects: the prisoners, in a state of despair, began by attempting to force open the door, but in this they were unsuccessful. Mr. Holwell, who was placed near one of the windows, was more at his ease than the rest, and was consequently more cool and tranquil; and he recommended his companions to be quiet and orderly, and not to exhaust their strength by useless efforts. This advice produced some little calm, interrupted however by the groans of the wounded and dying. The heat increased every moment. (This singular fact, viz. the increase of heat, when oxygen, the imaginary source of animal heat, was diminished, will be alluded to in its proper place.) Mr. Holwell recommended them to strip off their clothes, as the means of acquiring more space; this was accordingly done, but with no great relief; they attempted to improve this by fanning the air with their hats, but even this was too painful a task for men who were worn out by the fatigue of the siege and the heat of this dungeon. Another of the company was for their kneeling down, that they might have more air. They all readily agreed to do this, and to rise together in order to avoid confusion. This was done several times, but every time the signal was given to rise, the number of those who had strength enough to obey it diminished. There were constantly some remaining on the floor, who were unable to get up, and these were trodden to death by the survivors. All this happened during the first hour of their imprisonment. At nine o'clock in the evening they began to complain of excessive thirst, and to renew their efforts to open the prison door, and to tempt the sentinel to fire upon them. Some of those who were farthest from the window, became at once furiously delirious. The cry for water was unanimous. The guards brought water, and Holwell and two of his wounded friends received it at the window in their hats, and were going to pass it on to the rest; but so eager and tumultuous were the efforts of the crowd to get at this water, that Holwell's two friends were suffocated, the water was spilt, and Holwell saw himself surrounded by dead bodies, who had either been crushed to death, *or died for want of fresh air.* Hitherto the commander and benefactor of these unfortunate people, had been treated with some degree of respect, but now all distinction began to be forgotten. The whole company eagerly threw themselves *towards the windows*, and seizing the iron bars, some even got on his shoulders. He was so borne down by this enormous weight, as to be deprived of all power of motion; he implored the pity of those who were upon his head and his shoulders, and requested them to let him go, and die at the bottom of the prison; this request was readily complied with, as every one was desirous of succeeding to his place, and without much difficulty he reached the farther end of the dungeon. The third part of those unhappy people were already dead, and they who were still alive pressed so eagerly towards the windows, that Holwell found himself somewhat freer in his new station; but the air was so corrupted, that his breathing soon became extremely difficult and painful. Unable therefore to support this, he attempted once more to make his way to the windows; and, leaning on a heap of dead bodies, he now resolved to wait patiently for death. In this situation he remained about ten minutes, and then he experienced such a pain in the breast, and so violent a palpitation of the heart, that he was obliged to make one more attempt towards getting a less fatal air. There were five rows of his companions between himself and the window; his despair carried him through four of these. The palpitation of his heart now began to abate, but he felt inexpressible thirst, and cried out for water; but the water seemed to increase instead of alleviating his thirst; he therefore resolved to drink no more, and rather chose to suck the moisture from his shirt, which seemed to afford him some relief. A young man quite naked, who stood before him, eagerly seized the sleeve of his shirt, and for some moments deprived him of this salutary refreshment. It was not yet midnight. The small number of those who were left, were transported to the greatest excess of rage and despair. They all called aloud *for air*, because the water that had been brought to them afforded no relief. Soon after this the noise suddenly ceased. The greater part who were living laid themselves down deprived of all their strength, and peaceably breathed their last. Others aimed at getting into Holwell's situation; a Dutchman mounted on one of his shoulders, and a black soldier on the other. In this situation he remained till two in the morning, when he gave up his place to a marine officer, who was soon forced out of it by the Dutchman. The officer retired with Holwell to the other corner of the prison, and in a few minutes after died. Holwell himself was soon deprived of sense, and from that time till sun-rise we have no account of what passed. At a quarter after eleven o'clock, there came out of this melancholy dungeon three and twenty persons, the remains of the hundred and forty-six who had entered it on the preceding evening."*

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* On Inflammation.

The extent of this quotation might seem to require some apology; but the facts, both physiological and pathological, which it conveys, are so important, and will be so frequently referred to in the course of the following work, and the whole history is in itself so interesting and worthy of being recorded, that I may fairly expect, that my reader will not experience so much tædium in perusing as I have in writing it.*

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* Med. Jurisprudence.

The connection then between the supply of oxygen and the existence of living action is undoubted; and it is the opinion of some of our best writers, that oxygen contributes to the support of living action, not by acting as a stimulus, but by increasing the disposition of an organ to be excited into action. I shall have occasion to show, that the nervous influence contributes to the support of living action in precisely the same manner, and therefore I may presume that the nervous influence contains oxygen.

The phenomena of animal life must be intimately connected with the presence of oxygen in a free or combined state, because the absence of oxygen is *always* followed by a cessation of living action, and I am of opinion, that the errors which have been grafted upon this fact, may easily be traced, either to a want of discrimination between life and living action, or to the circumstance of those changes which the oxygen undergoes in its passage through the lungs, not having been sufficiently regarded.

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* It may be thought that I have introduced extracts from other works, into the body of my own, rather too liberally; but my *principal* object is to show, that the opinions I entertain are not inconsistent with the facts which have been recorded, and the arguments which have been advanced, by other writers; and by expressing my own opinions in the language of others, I express at the same time their concurrence in them.

I have already quoted the opinions of Galvani, Dr. Valli, Mr. Humboldt, Dr. Darwin, Sir Everard Home, Mr. Abernethy, Mr. Swan, and Dr. Wilson Philip, (and to these I might add a list of writers sufficient to fill this page,) all of which concur in regarding the nervous influence as *more like* electricity than any other element with which we are acquainted; and some have gone so far as to imagine, that they are the same things. If therefore I assume the fact, that electricity is a component part of the nervous influence, I can scarcely be said to deviate from those writers, who have imagined the nervous influence and electricity to be the same things; and if I assume the fact that the nervous influence is a modification of electricity, I can scarcely be said to deviate from those writers, who maintain that the nervous influence is *more like* electricity than any other element with which we are acquainted.

But I will venture one step further: I not only believe with those writers, that the nervous influence is a modification of electricity, but I believe that that modification is dependent upon the oxygen of the atmosphere. I might be justified in assuming this fact, if, by such an assumption, I could more satisfactorily explain the phenomena of living action; but many arguments may be adduced in favour of the supposition, that the nervous influence is a compound of oxygen and electricity. In the first place, if an animal lose all power of action, by being deprived of oxygen, we can, provided the vital principle be not extinct, by restoring oxygen, restore its power of action; there can, therefore, be no doubt, that oxygen contributes to the support of living action. Dr. Darwin says, "when we recollect that an electric shock will frequently stimulate into motion a paralytic limb, the opinion that a fluid, perhaps much more subtile than the electric aura, is separated from the brain, seems not without probability." The fact, then, "that an electric shock will frequently stimulate into motion a paralytic limb," is considered to be proved; and, indeed, I may state it as a fact, which is proved by daily observation, and which no one questions. Oxygen, then, and electricity, have equal claims to be regarded as the sources of living action. No author, I presume, is prepared to say, that oxygen and electricity are the same things; we may, therefore, presume, that living action is dependent upon both, and I shall endeavour, in the course of the following observations, to establish this fact; or rather to adduce such evidence, as shall render the fact extremely probable. In the present state of physiological science, we cannot hope to go much further. Some advantage may surely be derived from the attempt to reconcile the opinions of those physiologists, who attribute all living action to the influence of oxygen, with the sentiments of those who hold it "to be *demonstrated*, that they are electrical actions," which produce the various phenomena of animal life. This fact, if established, would also reconcile the opinions of those physiologists, who attribute the irritability of muscular fibre to the influence of duly arterialized blood alone, with the sentiments of those, who imagine the nervous influence to be the sole cause of irritability. They are both right and both wrong. Living action may be dependent upon oxygen *and* electricity, but not upon either exclusively. Millions of animated beings have been sacrificed upon the altar of experimental inquiry; and what is the result? We cannot at this moment determine, whether the action of the heart be dependent upon the arterial or the nervous influence, or whether it may not be dependent upon both. Is not this, at least, a presumptive proof, that the arterial and nervous influence may be the same things? In support of the opinion, that the nervous influence may be a peculiar combination of oxygen with electricity, I may adduce the example of galvanism. The fact is now established, beyond all power of contradiction, that if the nervous influence, supplying a muscle, be intercepted, that organ loses its power of being excited into action by the will of the animal.

(I shall have occasion, in a future part of these observations, to shew, that the whole phenomena of voluntary motion may be explained upon the supposition (which supposition is strongly supported by analogy and experiment) that the animal has the power of accumulating the nervous influence in that muscle which is thus made subservient to its will, and that this accumulation of nervous influence does *not* act as a stimulus, but simply creates an increased susceptibility of that action, which is the immediate effect of its *appropriate* stimulus). It is also proved by a variety of experiments, too numerous and too conclusive to admit of being questioned, that if the nervous influence supplying an organ, whose action is wholly independent of the will, (the stomach for instance) be intercepted by a division of its nerves, that organ loses the power of being excited into action by its appropriate stimulus. If, under these circumstances, the galvanic influence be applied to the inferior portion of the divided nerve, the organ, supplied by that nerve, *recovers* the power of being excited into action by its appropriate stimulus. The fact, as applied to voluntary muscles, is proved beyond all dispute. It is the foundation stone upon which the superstructure of all our reasonings, which are connected with the effects of galvanism upon the living frame, has been raised. The fact, as applied to the stomach, (certainly one of the most important organs in the body, and wholly unconnected with the will) has been established by the experiments and observations of Dr. Wilson Philip, and these have been repeated and confirmed by subsequent writers. He *fed* two rabbits, and then divided the whole of the nerves supplying the stomach of each. To the inferior portion of these divided nerves, in one rabbit, he applied the galvanic influence, and after a certain time opened the stomachs of both. In that stomach, the nerves of which had been subjected to the galvanic influence, *digestion* had clearly commenced ; but in the other stomach, the nerves of which had *not* been galvanized, nothing like digestion of the food could be observed. If these experiments do not point out the identity of, they at least establish the striking analogy between, the galvanic and the nervous influence. If, therefore, we have reason to suppose, that the galvanic influence may be a peculiar combination of oxygen with electricity, we have the strongest possible analogical proof, that the nervous influence is also a combination of these two substances. The nervous and the galvanic influence resemble each other in the effects they produce, and, therefore, we have the strongest possible reason for supposing that they resemble each other in the nature of the principles which enter into their composition.

However authors may differ as to the identity of the galvanic and electric fluids, they all admit (with some very few exceptions) that galvanism must, at least, be a modification of electricity. "The chemical changes produced by common and galvanic electricity, as far as they have hitherto been examined, are precisely similar." "All the effects of galvanic arrangements, in producing chemical decompositions, it has been found, may be obtained by common electricity."* "It is rather extraordinary, that the various discoveries of Galvani and others, relative to animal electricity, have not led to some more important information concerning the functions of the nervous system. Perhaps this may be owing to the attention of physiologists not having been properly directed towards the effects of the electric fluid on the animal fibre. "The late experiments of Volta and Cruickshank sufficiently prove, in opposition to the opinion of Humboldt and others, that the galvanic influence must be electricity, or a modification of it. The opinion that the electric fluid is that power whereby the functions of the nervous system are performed, has had some adherents, ever since the discovery of the electrical powers of the Torpedo and Gymnotus Electricus; but there is a book, published more than thirty years since, and probably written long before its publication, in which the identity of the electric and nervous fluids is hinted at."*

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* Henry's Chemistry.

It will, I conceive, be readily admitted, that galvanism, if not electricity itself, must at least contain that fluid, as one of its constituent principles; and yet, there are the strongest reasons for supposing, that the electric and galvanic fluids are not the same things. M. Humboldt says, "Old well-dried bleached bones are as good, and in part *better* conductors of electricity, than the metals; but *insulate* galvanism as perfectly as glass or sealing wax.

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* Med. & Phys. Journ. 1801.

"Rarefied air conducts the weakest degree of electricity, but not galvanism.

"Flame is the most perfect conductor of electricity, but insulates galvanism.

"Warm glass conducts electricity perfectly, but insulates galvanism."*

"With regard to quicksilver, Dr. Fowler has taken notice of a remarkable singularity: if the abdomen of a frog be filled with it, a piece of *zinc* passed through it, so as to touch the sciatic nerve, excites contractions; but a piece of *silver* passed to them excites none. Neither are any excited by touching the silver beneath the surface of the mercury with a piece of zinc. When water is used instead of mercury, contractions may in this way be excited; yet mercury is reckoned a much better conductor of electricity than water."†

"Dr. Fowler next examines the effects of temperature upon the conducting power of different substances. By a red heat, it was not diminished in zinc or iron; nor, by its influence, did a crucible or piece of coal become conductors. Contrary to the assertion of Dr. Valli, our author found, that the conducting power of water was not affected by any increase or diminution of its temperature. In the scale of conductors of electricity, charcoal holds a higher place than the fluids of animal bodies, and ice than the metallic salts. Of the influence in question, animal fluids and metallic salts are excellent conductors; while charcoal, dried wood, and ice refuse it a passage."

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* Annals of Medicine, 1798.

† Duncan's Med. Comment. 1793.

"But the most important and characteristic difference, which, Dr. Fowler observes, he has been able to discover between this new influence and electricity, consists in their effects upon the contractile power of animals and of plants. He has found, that so far from producing exhaustion, the more frequently contractions have been excited by this influence, the longer do they continue excitable, and the longer are the parts upon which such experiments are made, preserved from putridity."* Even those authors who assume the identity of the galvanic and electric fluids, are under the necessity of admitting, that the effects produced by each are not precisely similar. "When a piece of watch pendulum wire is placed in the circuit of a common electrical battery, containing not less than three or four square feet of coating, at the moment of the discharge the wire becomes red hot; but continues so *only for a few seconds*, no longer indeed, than if it had been ignited in any other way. The same effect may be produced by making a piece of wire the medium of communication between the opposite extremities of a galvanic trough; but in this case, the heat continues *sensibly longer* than when it is excited by an electrical explosion."*

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* Med. Comment. 1793.

"When the power of an electrical battery is increased, metallic wires, by transmitting the discharge through them, may be melted, and dispersed in the form of smoke, or of an impalpable powder lighter than air. The galvanic discharge, also, is capable of *fusing* metallic wires; but *being less violent*, it does not scatter their particles to a distance."†

"Though the identity of common and galvanic electricity appears to be sufficiently established; yet, in *some* of their phenomena, which have already been described, there is a considerable difference. To explain these, it was long ago *suggested* by Mr. Nicholson, that the electricity excited by the common machine, is developed in much smaller quantity, but in a higher state of concentration or intensity than the electricity of galvanism." To corroborate the probability of this suggestion, it is stated, that "the comparative quantities of electricity evolved by the common machine and a voltaic apparatus, have been made a subject of calculation by Mr. Nicholson. A pile, consisting of 100 half crowns, with the same number of pieces of zinc, produces, he found, 200 times more electricity than can be obtained in an equal time from a 24-inch plate machine in constant action."* But why should electricity, in a concentrated state, pass less readily through water, than electricity in the state of galvanism? We are told, that, in consequence of this concentration, "its velocity is much more rapid; and hence it *readily* passes through plates of air and other non-conductors, that are scarcely permeable by galvanic electricity;"† and yet, the passage of galvanic electricity, through water, is *more* rapid, than that of electricity in its more concentrated state. This observation particularly applies to the *animal fluids*, considered as the conductors of these two substances. I cannot help thinking that the theory, which regards the galvanic fluid as a modification of electricity, is the most satisfactory hitherto suggested.

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* Henry's Chemistry. † Ibidem.

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* Henry's Chemistry. † Ibidem

Upon what does this modification depend? We know that the galvanic fluid cannot, under any circumstances, be artificially excited without oxygen. "Hence it appears, that the oxidation of one or both of the metals composing the trough, is essential to the excitement of galvanic electricity."* The first question which naturally suggests itself is this :—May not the galvanic fluid be a peculiar combination of oxygen with electricity? May not galvanism be regarded as *oxygenated* electricity? The theory proposed by Mr. Cruickshank approaches very near to this: "He thinks that the simplest and easiest mode of explanation would be, to suppose that the galvanic influence *(whatever it may be)* is capable of existing in two states, that is, in an *oxygenated* and *de-oxygenated* state. That when it passes from metals to fluids containing oxygen, it seizes their oxygen and becomes oxygenated; but when it passes from the fluid to the metal again, it assumes its former state, and becomes de-oxygenated. Now when water is the fluid interposed, and the influence enters it from the silver side de-oxygenated, it seizes the oxygen of the water, and disengages the hydrogen, which accordingly appears in the form of gas; but when the influence enters the zinc plate, it parts with the oxygen with which it had formerly united, and this either escapes in the form of gas, or unites with the metal to form an oxyd, or combined with some of the azote contained in the water, may form nitrous acid."* Is that theory which has been suggested by later experiments, and proposed by subsequent writers, more satisfactory? "But though the *chemical* agency of the fluids which are employed is now admitted, on all hands, to be essential to the excitement of *this kind* of electricity, yet it is by no means universally agreed that we are to consider it as the first in the order of the phenomena. It has been suggested by Mr. Davy, as a correction of the theory of Volta, that the electro-motion occasioned by the contact of metals, is the primary *cause* of the chemical changes; and that these changes are in no other way efficient, than as they restore the electric equilibrium. To explain this, let us suppose that in any three pairs of plates of a galvanic trough, the zinc plates are in the state of positive, and the copper plates in that of negative, electricity. The liquid in any cell after the first will be in contact, on the one side, with positively electrified zinc, and on the other side with negatively electrified copper. And if the elements composing the fluid be in different states of electricity, the negatively electrified will be attracted by the zinc, and the positively electrified element by the copper. Thus when solution of muriate of soda in water is the fluid, the oxygen and acid will pass to the zinc, or positive plate, and the alkali to the copper one, while the hydrogen, having no affinity for copper, escapes. The electric equilibrium will be restored, but only for a moment, for as the interposed fluid is but a very imperfect conductor of electricity, the zinc and copper plates will, by their electro-motive power, again assume their states of opposite electricity, and these changes will go on as long as any muriate of soda remains undecomposed."* Does this theory at all account for the *accumulation* of electricity at the zinc end of the apparatus? Does it at all account for the absolute necessity of *oxygen* to the existence, or rather to the excitement of the galvanic influence?

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* Henry's Chemistry.

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* Med. and Phys. Journ.

Is the theory proposed by Dr. Bostock at all more satisfactory? "He takes it for granted that the electric fluid has an affinity for hydrogen, and supposes that the electricity evolved at the surface of the first zinc plates, is carried, united to hydrogen, through the fluid of the cell to the opposite copper-plate. Here the hydrogen and electricity separate; the former flies off in the state of gas, and the latter passes onward to the next zinc plate."* The oxygen evolved by the decomposition of the water, is, I presume, supposed to unite with the metal, and to produce its oxidation. The necessity of oxygen to this process, is supposed to be explained by the assumption that, " the oxidation of the zinc is the source of the evolved electricity." It is not necessary to inquire more particularly into the merits of these different theories. Dr. Henry says, "Such are the *hypotheses* that have been framed to explain the phenomena of the voltaic pile. In the present state of the science, *neither* of them is entitled to be regarded as satisfactory." It would be wholly irrelevant to my subject, to involve myself in any discussion as to the chain of actions by which galvanic electricity is excited; all that it is necessary for me to establish is this, that electricity or galvanism may and do, consistently with the theory proposed by Mr.

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* Henry's Chemistry.

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* Henry's Chemistry.

Cruickshank, occasionally exist in combination with different quantities of oxygen, and that we are justified in using the terms of oxygenated and de-oxygenated electricity or galvanism. In the decomposition of water, at the close of the experiment, "oxygen gas is found in the leg connected with the positive end of the battery, and hydrogen gas in that connected with the negative end; and in the proportions which, by their union, compose water."* If the electric equilibrium between oxygen and hydrogen, united so as to constitute water, had not been disturbed, no decomposition of the water could have taken place; oxygen, therefore, is capable of combining with different portions of electricity; that is, it may contain more electricity at one time than another in its composition. When, therefore, oxygen contains more than its natural quantity of electricity, the latter may be said to be de-oxygenated, because the quantity of oxygen, compared with the quantity of electricity, is diminished; on the other hand, when oxygen contains less electricity than in its natural state, the latter substance may be said to be oxygenated, because the quantity of oxygen, compared with the quantity of electricity, is increased. The oxygen given out at the positive end of the wire, must be supposed to contain more or less of electricity in its composition, than it did in its natural state, otherwise it would never have arrived at the end of the wire, but would have united with the hydrogen and have constituted water, and this fact is confirmed by subsequent experiments, by which it appears, that an acid will pass through an alkali without uniting with it, in consequence of their electric equilibriums having been disturbed. It may be said, that the electrical state of the hydrogen only is altered, and that the equilibrium is destroyed by this circumstance alone ; but this is assuming a fact, which it would be extremely difficult to prove, and which appears to be in direct opposition to the following experiment: "It has been established by Mr. Davy, that when a galvanic arrangement of the second kind, is constructed, by alternating metallic plates with strata of different fluids, alkaline solutions always receive electricity from the metal, and acids on the contrary transmit it *to* the metal. If then the alkali, after having acquired electricity from the metal, could be suddenly separated from the combination, there can be no doubt that it would be found in a positive state. For the contrary reason, the acid, having *given* electricity to the metal, must, if it could be detached, be found negative."* The *acid*, then, is capable of existing in a positive and in a negative state. As oxygen is the principle of acidity, we may conclude, that oxygen is also capable of combining with different quantities of electricity; and, if so, the electricity may be said to be more or less de-oxygenated, in proportion as the quantity of oxygen, compared with the quantity of electricity, is deficient or in excess.

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* Henry's Chemistry.

As I have suggested the idea, that the arterial influence may be very nearly allied to oxygenated electricity, it seemed of consequence to point out the probability of the theory suggested by Mr. Cruickshank; but the analogy between the arterial and galvanic influence is sufficiently striking, when we consider that oxygen is essential to the existence or the excitement of both. The analogy is still farther pointed out by experiment. The peculiar property of the arterial influence, is to change the dark hue of venous, into the scarlet colour of arterial blood. The galvanic influence will also produce this change in the colour of venous blood. If the wires, attached to each end of the galvanic trough, be brought into contact, or into a certain state of approximation, under the surface of venous blood, bubbles of a vivid scarlet hue immediately rise to the surface. As often as the experiment is repeated, the same phenomenon may be observed. If a small portion of the dark coagulum of venous blood be allowed to attach itself to each wire, that of the positive wire will assume a brighter appearance, and that of the negative wire will be little, if at all, altered in its colour; but as soon as the two wires are brought into contact, the coagulum attached to each will assume a bright scarlet colour. These facts, however, do not rest upon my own authority alone. Mr. Wilkinson says, "venous blood, when exposed to the galvanic process, soon acquires the redness of arterial blood;"* and Mr. Brande has recorded the following experiment, which confirms the observation made by Mr. Wilkinson, and which exactly corresponds with my own experiments, as far as my attention was directed to the *colour* of the blood:—"The conductors from twenty-four 4-inch double plates of zinc and copper, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been with-drawn from a vein in the arm. The temperature of the blood was kept up at 100 during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a *deep red* colour, and extremely alkaline ; that surrounding the positive wire was slightly acid, and of a *brighter hue*."* It is of no consequence in what manner we explain the fact, that the galvanic influence has the power of imparting to venous, the bright colour of arterial blood. The fact is fully established, and that, at least, is sufficient to substantiate a further analogy between the galvanic and the arterial influences, as far as their powers, in affecting the colour of the blood are concerned. It may be said, that the colour of the blood is in both instances affected by oxygen, and that no analogy is thus established, as to the *electrical* properties of the arterial and galvanic influences. But if, in our attempts to explain the phenomena of living action, we shall render it probable, that the arterial influence does contain electricity, the analogy will then be established, as far as the *electrical* properties of each are concerned.

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* Henry's Chemistry.

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* On Galvanism.

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* Philosoph. Transact. 1809.

We know that the action of the brain is dependent upon a due supply of arterial blood. This is proved by the circumstance, that a loss of sensation is always the consequence of this supply being interrupted. We know also, that muscular action is dependent upon a due supply of the arterial influence; and this is proved by the circumstance, that a ligature applied to the main artery of a muscle, will always deprive that muscle of its power of action. If, for instance, the supply of arterial blood through the descending aorta be interrupted by ligature, the inferior extremities *always* become paralytic. " En effet, si l'on répète sous ce point de vue une experience rapportée ci-devant, laquelle avoit déjà été faite par *Stenon*, et qui consiste a lier l'aorte sur la premiere vertèbre lombaire, nous avons vu que peu après *le sentiment et le mouvement* disparoissent entièrement dans le train de derriere, pendant que la circulation et la vie continuent dans les parties anterieures; mais si, après avoir attendu un temps triple et même quadruple de celui, au bout duquel tous les signes de vie ont disparu, on delie l'aorte, le sentiment et le mouvement reunissent peu à peu dans les parties meurtes, à

mesure que la circulation s'y retablit."* The power of motion, then, in the inferior extremities, is not only suspended by intercepting, but is also renewed by restoring the supply of arterial blood. There are different modes of explaining this fact, which I shall consider in a future part of these observations; but, at present, I am only called upon to record the fact itself. We find, then, that the arterial influence will, under certain circumstances, restore to an organ its power of action; and as the acknowledged effect of the arterial influence is to support the irritability of muscular fibre, we must presume, that in the circumstances alluded to, and actually recorded by M. Le Gallois, the inferior muscles lost their irritability in consequence of the ligature upon the aorta, and that, upon the removal of this ligature, they recovered this property, in consequence of the due supply of arterial blood being thus restored. The arterial influence does not act as a stimulus to muscular fibre, but only contributes to the support of that peculiar property, by which muscular fibre is rendered capable of being excited into action by its appropriate stimulus, which is *not* the arterial influence. It is mentioned by Dr. Darwin (in the passage already quoted) as a fact which does not admit of being questioned, that electricity is capable of stimulating a paralytic limb into action. There may be a question as to the mode in which this action is excited. It still remains to be proved that electricity acts as a stimulus to, and does not act by simply contributing to the irritability of, the paralytic muscle. But it is the fact only which I wish to record ; and the fact is simply this, that electricity being applied to a paralytic muscle, action is the consequence. The effect of galvanism in producing muscular action, under certain circumstances, is fully established ; and I shall endeavour, in a future part of these observations, to show that galvanism produces this effect, not by acting as a stimulus to muscular fibre, but simply by contributing to the support or increase of its irritability, that is, its susceptibility of action.

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* Le Gallois.

The analogy between the electric and nervous fluids is admitted by some of our best physiologists. "The nervous fluid, if not electricity itself, is *more like* that substance than any other with which we are acquainted."* Living action is most intimately connected with a due supply of the nervous influence ; and one author† has gone so far as to say, that he considers it to be *demonstrated*, "that they are electrical actions," which produce the various combinations and decompositions observed to take place in the animal frame. Such an opinion seems, at least, to imply a demonstrated analogy between the nervous and electric influences.

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* Swan on the Nerves.

† Mr. Abernethy, whose acknowledged ability, if not a passport to any opinion which he may offer, has, at least, a claim upon our serious consideration.

The analogy between the nervous and galvanic influences is still more striking.

Oxygen is *essential* to the excitement, and probably to the existence, of both.

They both change the dark hue of venous, into the scarlet colour of arterial blood.

They both exert a remarkable influence over living action.

They both influence living action in the same manner; that is, by contributing to the irritability of, and not by acting as a stimulus to, muscular fibre.

Living action has been attributed by one class of physiologists to oxygen, and by another to electricity; and these substances are both concerned in the excitement, if not in the constitution, of the galvanic influence.

There is reason to suppose that galvanism is a modification of electricity; and it seems to be not improbable, that that peculiar modification is dependent upon oxygen. Electricity and oxygen are both present in the animal frame, and I shall endeavour to show that the phenomena of living action are not inconsistent with the idea, that the arterial and nervous influences are composed of these two substances.

What are the most striking points of analogy between the nervous and arterial influences? Oxygen is essential to the existence of both. The fact, as connected with the arterial influence, is admitted by all physiologists. The oxygen lost in respiration is imagined, by some few authors, not to unite with the blood; but all admit, without exception, that oxygen is essential to the *excitement* of the *arterial* influence. Some few authors have imagined that oxygen forms a component part of the *nervous* influence, but all admit, (without exception, if we confine the observation to modern physiologists), that oxygen is essential to the *excitement* of the *nervous* influence. "La vie, (by which expression the author evidently means the nervous influence) est due à une impression du sang *arteriel* sur le cerveau et la moëlle epinière, ou a un principe resultant de cette impression."* A very large proportion of blood, possessing those characters which we attribute to the influence of oxygen, is sent to the brain, and is returned from thence deprived of those peculiar characters. The same arguments which lead us to suppose that oxygen unites with the blood in its passage through the lungs, might lead us to presume, that the blood, in its circulation through the brain, imparts oxygen to the nervous influence. This fact may be questioned ; but few physiologists will hesitate to admit, that the oxygen of arterial blood is essential to the *excitement* of the nervous influence.

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* Le Gallois.

The nervous and arterial influences affect living action in a manner so similar, that the arguments, the assumptions, the conjectures, the experiments, of more than half a century, have not enabled us to determine upon which the action of the heart is dependent. Dr. Wilson Philip has communicated to the medical profession, an account of some very important experiments, which were instituted for the purpose of supporting the doctrine of Baron Haller ; viz. that the action of the heart is independent of its nerves ; but even this author is under the necessity of admitting, that the action of the heart is *occasionally* influenced by the nervous energy. If this occasional influence of the nerves produce the *same* effect upon the action of the heart, as is attributed to the more permanent influence of arterial blood, may we not presume, that the effect being the same, the cause is also the same? Or, supposing this to be questioned, is it possible to deny the *analogy* between two causes, when the effects produced are, at least, apparently the same, and are not proved to be otherwise? A very important physiological question presents itself to our consideration :—Is the nervous influence capable of affecting the colour of the blood? Blushing consists in a chain of actions, the first link of which evidently originates with the mind. Dr. Darwin says, "The blushing of accused people, whether guilty or not, appears to be owing to circumstances similar to that of anger; for in these situations there is always a sudden voluntarity, or wish of clearing their characters, arises in the mind of the accused person, which, before an opportunity is given to it to be expended on the large muscles, influences the capillary arteries and glands, as in the preceding article. Even in the blush of modesty, or bashfulness, there is a *self-condemnation* for some supposed defect or indecorum, and a sudden voluntarity, or *wish*, of self-defence."* There can be no doubt that blushing originates with the mind, and if with the mind, with the nervous system. The only point in question is this: whether an increased action of the nerves, or any change in their state of action, can *of themselves* produce the blush of anger, modesty, &c.; or whether blushing be the immediate effect of an increased action of the blood vessels, or any change in their state of action, dependent upon the nervous influence. The latter theory is that which has been most generally adopted. "The cutaneous capillaries, especially those of the face, are more mobile, that is, more easily excited into increased action, or more easily become torpid from less variation of sensorial power, than any other parts of the system, which is owing to their being perpetually subject to the vicissitudes of heat and cold, and of extension and corrugation. Hence when an excess of voluntarity exists, without being immediately expended in the actions of the larger muscles, the capillary arteries and glands, acquire more energetic action, and a flushed skin is produced, with increased secretion of perspirable matter, and consequent heat, owing to the pause or interruption of voluntary action; and thus the actions of these cutaneous vessels become associated between the irascent ideas and irascent muscular actions, which are thus for a time interrupted."* In another part of his work, the same author says, "In a manner somewhat similar to this, is the redness of the skin produced in angry people by the *superabundance of the unemployed sensorial power of volition.*" It is not easy to catch the precise meaning of the author, but I conceive it to be this; that there is more sensorial power, that is, more nervous energy thrown upon the part by an act of volition, than the organs can employ. How does this superabundance of the unemployed nervous energy produce the blush of anger, modesty, self-condemnation, &c? It is supposed to excite an increased action of the capillary arteries. Dr. Darwin says, " The blush from heat is occasioned by the increased action of the cutaneous vessels, in consequence of the increased sensation of heat. The blush of joy is owing to the increased action of the capillary arteries from the increase of pleasurable sensation."* Can we, upon this supposition, satisfactorily account for the phenomenon observed? How does increased action of the capillary arteries produce the blush of anger, modesty, &c.?

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* Zoonomia.

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* Zoonomia.

The natural action of the arteries is to contract upon their contents, and to *diminish* their capacities. It is clearly impossible that an artery should contain *more* blood in consequence of its capacity being *diminished.* How have authors attempted to reconcile this palpable contradiction? Mr. John Hunter says, "The act of inflammation is to be considered as an increased action of the vessels, which at first consists simply in an increase or distension beyond their natural size; the elastic power of the artery must be dilated in the same proportion. This is therefore something more than simply a common relaxation. We must suppose it an action in the parts to produce an increase of size to answer particular purposes, and this I should call an action of dilatation."* This is not a very clear mode of expressing the opinion, that the elastic power of an artery to dilate itself, may be superior to that power by which it contracts upon its contents. It is also assuming a fact which is very questionable, that the coats of an artery are *capable* of that action, by which their calibre is *enlarged;* that is, that in their dilatation they are not merely passive, and yield to the distending power of the increased volume of blood impelled into them by the *vis a tergo*, but that they dilate themselves by their own action. If we admit this fact, we must necessarily admit another; viz. that the increased size of the artery is owing to a *diminished* action of the natural and contracting power of the artery, *compared* with that of its dilating power. When, therefore, we say that an artery is capable of having its calibre enlarged, and attribute that enlargement to an increased action of the vessel, (which is generally supposed to mean an increased action of its contracting power, and which, if it does not mean this, is assuming an imaginary property of an artery, to account for what is more simply explained, by attributing the increased dilatation to the increased volume of blood, which is impelled into the artery by the vis a tergo), we convey a false idea, because it is, in reality, owing to a *comparative diminution* of that power which we represent as being increased. Many authors, influenced by the consideration of this difficulty, have attributed the act of inflammation to a partial debility of the vessels, by which they more readily yield to the influx of the blood. Mr. Burns says, "Partial debility of the vessels cannot, on the other hand, produce the second species of inflammation, because this debility should at the very utmost, produce only a slower inflammation, and not an inflammatory action. (It is not easy to comprehend the difference between an action of inflammation and an inflammatory action.) Neither ought it to be the cause of swelling and turgidity of the arteries, by allowing more than a due quantity of blood to be forced into them, for the quantity of blood in a vessel does not depend upon a proportion betwixt its contractile power, and the propelling power of the rest of the system, but upon its own action entirely, otherwise we should find paralytic limbs always turgid with blood."* It is not easy to account for the œdema which often supervenes upon a paralytic limb, without admitting that a turgidity of the blood vessels is, or has been, present; and the argument is equally strong upon the other side. If the quantity of blood in a vessel be always "dependent upon its own action entirely," as the action of a paralytic vessel is diminished, the quantity of blood contained in that vessel ought also to be diminished. Is the author prepared to show that this is the fact? Have not other authors shown that it is not the fact? If increased action be the cause of increased dilatation of an artery, how does it happen, that by the application of a stimulus to a vessel surcharged with blood, in opthalmia for instance, we diminish the quantity of blood contained in it? We diminish the calibre of a vessel by exciting that very identical action which, according to this hypothesis, ought to increase it.

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* Zoonomia.

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* On Inflammation.

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* On Inflammation.

Admitting that increased action of a vessel is capable of adding to its size, and consequently to the quantity of blood circulating in it, is redness always the consequence? Dr. Parry says " Since, however, we find that in gout in the foot, the *œdema* often continues to extend itself in proportion as the inflammation in the foot subsides, nay, sometimes exists in the greatest degree and to the greatest extent, long after all symptoms of local inflammation have subsided, we cannot help attributing its occurrence, in the latter case, to such a state of *momentum of blood* still existing in the vessels leading to the part originally inflamed, as produces præternatural evacuation by exhalation. This being admitted we may reasonably suppose, that the anasarca or œdema stated to have arisen to a considerable distance above the local inflammation, originated in a *similar* condition in the neighbouring arteries and exhalants ; *both* of which appear, from all the phenomena in such cases, to be *præternaturally distended with blood*."* It appears then that œdema, a colourless swelling, arises from the same cause as inflammation, that is, from the same cause, as far as the circulation is concerned. There is no difference in the circulation of a part under these two circumstances, but there is a manifest difference in its sensibility. I appeal to any one who has recovered from a paroxysm of the gout, and is still labouring under œdema, as the consequence, whether I have stated the case fairly. Will this difference in the sensibility of a part account for the difference in its appearance? Dr. Parry says, "From this fact it follows, that the *increased momentum*, which is a leading circumstance in the phlogistic diathesis, is sufficient to produce anasarca or œdema without the existence of local inflammation, as the source from which the effusion takes place. The difference between œdema and inflammation does *not* depend upon *any* variation in the quantity or the momentum of the blood, but the obvious difference is a *change of colour and increased sensibility* in a part affected by the latter."* It appears, then, that we cannot account for the phenomena of inflammation (" Partis externæ *Rubor*, *Calor* et *Tensio dolens*,) and those of an œdematous swelling (" Phyma *album*, *frigidum*, *indolens*,) without admitting that the vessels are, in both these diseases, equally

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* Elements of Pathology.

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* Elements of Pathology.

enlarged and equally *turgid* with blood; and yet the characters of these diseases are in direct opposition to each other. Heat, redness, and pain, characterize the one; coldness, whiteness, and insensibility, the other. According to Dr. Darwin's theory of blushing, an accumulation of unemployed sensorial power (that is, of nervous energy or nervous influence) takes place in a part. It is hardly assuming a fact, to say, that this occurs in inflammation, because we cannot account for the increased *sensibility* of an inflamed part, without admitting the fact. If this nervous energy bear any relation to the arterial influence, we at once see why the accumulated unemployed sensorial power, by *uniting with the blood*, may produce that change in its colour which we are endeavouring to explain. There is no difficulty in accounting for the increased sensibility of the part; and the experiments of Mr. Brodie, having pointed out a most intimate connection between the action of the nervous system and the development of animal heat, there is no difficulty in accounting for the increased heat which we observe. Consistently with this idea, there is, in œdema, a *deficiency* of the sensorial power or nervous energy, and therefore all the symptoms are reversed. We thus see why, in an inflamed part, there may be an increased action of the vessels, a diminution of their capacities, and even a smaller quantity of blood circulating in them, (the quantity of blood circulating *through* a vessel in a given time, may, in consequence of its increased velocity, be greater, although the quantity contained in it at any given time may be smaller,) and yet that part may be characterized by increased redness, owing, not to the quantity, but to the quality of the blood. It may be said, that blood drawn from a *vein* in inflammation, does not indicate increased redness; but I shall have occasion to shew, that the blood, in its circulation through the system, is constantly parting with its arterial influence, by which its colour is changed, and that this is more particularly the case in inflammation. Assuming the identity of the arterial and nervous influences, there can be no difficulty in answering the objection which I have suggested.

Is there any reason to suppose, that the nervous influence does, under any circumstances, unite with the blood? "Dr. Fowler could obtain no contraction by coating the crural arteries of frogs, while the blood still flowed through them, although the electric spark, or even a full stream of the aura, passed into the artery, produced them."* This observation would seem to imply, that electricity (and by analogy, the nervous influence) does not unite with the blood; but it only proves that the coats of an artery are better conductors than the fluid which they contain; it does not prove that if electricity (and by analogy the nervous influence) were *accumulated* in the coats of that artery, it would not *then* unite with the blood; neither does it prove that the nervous influence, which is only supposed to be a *modification* of electricity, must be obedient to the same law. It is not even stated, that the colour of the blood was not altered. It is perfectly consistent with that law of the animal economy which I have endeavoured to establish, (viz. that no substance having performed its destined function in the system, is ever called upon to repeat that function, but immediately becomes excrementitious) to suppose that the nervous influence, having supported the increased action of the artery, and then united with the blood, is no longer capable of supporting living action. If it shall appear, that the action of an organ is always attended with an evolution of oxygen, (a fact which I shall hereafter endeavour to establish,) there is no difficulty in conceiving, that the action of an artery, by giving out oxygen, will change the colour of the blood.

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* Med. Comment. 1793.

The florid colour of the sanguineous temperament, is usually attributed to an active state of the capillary arteries, and occasionally to the greater transparency of the skin. But if this be the cause, why is not the dark colour of venous blood the predominant character? The capacity of the veins is much greater than that of the arteries; the quantum of venous exceeds that of arterial blood; and therefore, in proportion to the transparency of the skin, the dark hue of venous blood ought to predominate. Mr. Burns says, " In some diseases, we find that the nerves do not take from the blood as much *life* as it is capable of yielding. In this case the blood in the veins is much *redder* than usual, and when drawn, coagulates, almost totally, into a mass, containing, like the coagulum of arterial blood, the greatest quantity of the serum blended with the other principles. In many diseases proceeding from the derangement of the stomach, and bowels, and, in approaching syncope, this fact may be observed."* The observations of Mr. Brodie and others seem to coincide with those of Mr. Burns, although the fact is explained in a different manner. " After death *(from exposure to excessive cold)* the blood is generally florid in the aorta, so that the animal does not die of suffocation; the heart sometimes contracts feebly, after the muscular irritability of the limbs and intestines are nearly destroyed; the cerebral veins contain but little blood; the ventricles contain a small portion of fluid. Mr. Brodie's experiments coincide, in most respects, with those of Dr. Chassat, who uniformly found, after death, the heart much distended with blood, as in syncope—scarlet blood occupying the left side; and he also found that the heart ceased to contract before the diaphragm, so that he has seen the animal insensible, and gasp for breath, *even after the chest was opened and the heart excised!* The *muscles* were unusually *florid.*"* Now, although the scarlet colour of the blood in the left side of the heart, may *possibly* be owing to the action of the heart ceasing before that of the respiratory organs, the florid colour of the muscles cannot be attributed to the same circumstance; if the blood stopped in the left side of the heart, it could not be conveyed to the muscles. The fact seems to admit of an easy explanation. The action of the nervous was disproportioned to that of the circulating system, and the latter conveyed more oxygen to the blood than the former took from it. The increased redness of the blood is thus the consequence of diminished, not increased, action of the nervous system. "The nerves do not take from the blood as much life (that is, according to my view of the subject, as much arterial influence) as it is capable of yielding." I have seen this fact exemplified in bleeding. The operator missed the vein, and a few drops of *dark* venous blood escaped from the cutaneous vessels. The circumstance created some degree of alarm in the patient, and the blood, which continued to escape in small quantities from the same vessels, was of a scarlet colour.

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* On Inflammation.

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* Med. Jurisprudence.

In infants, the nervous system is more fully developed than in adults; the nervous system, therefore, ought to take more life from the blood, and that ought to be less florid than in adults. But the reverse of this we know to be the case. It cannot be supposed that, in infants, the "vitality of the air" is supplied in greater quantity by the lungs than it is expended by the nervous system, because, the disproportion between the lungs and the nervous system is greater in infancy than at any subsequent period of life. The lungs are more contracted, and the nervous system is more expanded, than at any subsequent period, and therefore, we may presume, that the action of the latter is more energetic than that of the former. But if the increased action of the arterial system, and the consequent expenditure of the nervous influence, be themselves sources of oxygen, we have no difficulty in perceiving how the florid colour of the blood may, in one instance, be the effect of diminished, and in the other, the effect of increased, action of the nervous system. I am inclined to suppose, that, consistently with this view of the subject, the petechiæ of fever, the phenomena of scurvy and purpura hæmorrhagica, and the imperfect oxygenation of the blood, which may frequently be observed in maniacal complaints, and which does not appear to have been regarded with that attention which its importance demands, may be more satisfactorily explained.* If we admit this connection between the colour of the blood and the action of the nervous system, and the theory which attributes the redness of inflammation to an increased action and an increased capacity of the blood vessels, be inadmissible, because this state of the vessels occurs in œdema where there is no redness; may we not presume, that the redness of inflammation is intimately connected with the state of the nervous system, and that it is more dependent upon the quality than the quantity of blood contained in the circulating vessels?

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* I have so frequently observed this state of the circulation in melancholia, and in some cases of mania, where it could not be attributed to any defect in the lesser circulation, that I am disposed to regard it as a very important symptom in affections of the mind.

How, it may be asked, does increased action of the vessel render the blood more florid? We find, that under all circumstances, great muscular action is attended with a respiration quicker than natural. More oxygen is evidently taken into the system, and, therefore, we might *a priori* conclude, that increased muscular action is attended with an increased expenditure of oxygen. Dr. Paris says, "It has been satisfactorily established by physiological researches, that the demand for oxygen, in an animal body, will be in proportion to its expenditure by muscular exertion."* Increased action of the blood vessels, therefore, must imply an increased expenditure of oxygen. Why may not this unite with the blood, and produce that change in its colour which characterizes inflammation? It may be said that the colour of that blood, which is drawn from the arm by venesection, in cases of inflammation, is not præternaturally red. But this fact is sufficiently accounted for by the circumstance, that the inflammatory affection is only local. The nervous system is imparting oxygen to the blood in one circumscribed part of the animal frame, whilst the generally increased action of the nervous system is taking oxygen from the blood in every other part. Without attaching much importance to the theory here suggested, I may be allowed to say, that the idea is not, in the least, *more hypothetical*, than that which attributes to an artery the power of dilating itself, and of enlarging its calibre, at a time when we have evidence of that action being increased, which, under other circumstances, diminishes it.

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* Med. Jurisprudence.

The *sudden change* which is produced in the properties of the blood, during the operation of bleeding from the arm, would seem to be more connected with the nervous, than with the arterial system; for we cannot, by any attention to the state of the pulse, during the operation, say beforehand, that the first cup will be buffed, the second and third not at all so, and that the fourth will again separate the inflammatory crust.

The actions of the arterial and nervous

systems are so intimately blended, that the change, which is attributed by one practitioner to the nervous, will, by another, be referred to the arterial system; but, when the chain of actions evidently commences with the former, as in blushing, the theory which attributes the phenomena observed, to the nervous system, would appear to be the most probable. Why may not "the superabundant unemployed sensorial power" unite with the blood immediately? or, having contributed to the support of arterial action, why may it not then unite with the blood? If the sensorial power and the arterial influence be the same thing, the increased redness, which constitutes blushing, would appear to be the necessary consequence of this union. If the parts of the system which are remote from the heart, are less dependent upon the arterial, and more dependent upon the nervous, system, (the probability of which I shall have occasion to point out), we shall have an additional reason for attributing the blush of anger, modesty, &c. to "the superabundant unemployed sensorial power," and shall, in some degree, account for that sensation which is termed "blushing to the fingers' ends."

I might adduce many examples in support of the fact, that the colour of the blood is often changed by local affections of the nerves; but I should not, by so doing, advance my argument, because the nervous and arterial systems are so united in their actions, that what I might attribute to a positively diminished action of the former, would, by another, be referred to a sympathetically diminished action of the latter. If the nervous influence does, under certain circumstances, unite with and produce that change in the colour of the blood, which, under other circumstances, is attributed to the arterial influence, a further analogy between the two is thus established.

It is universally admitted, that the action of the heart is either directly, or indirectly, connected with the arterial influence; and the physiologists of the present day, more particularly Bichât, imagine that the arterial influence unites with the heart, and contributes to the support of its irritability, by which it becomes sensible to the impression of its appropriate stimulus—the blood. The old theory, which imagined the two sides of the heart, similar in structure and function, to be excited into action by different stimuli, is thus abandoned; but a difficulty presents itself *in limine*. Supposing the action of the heart to be suspended, or even diminished, how could its power be restored under any circumstances? The power of the heart, according to this theory, is wholly dependent upon itself; that is, upon the supply of the arterial influence, through the medium of the coronary arteries, which supply pre-supposes an action of the heart itself. Thus the heart must act, in order to acquire the power of doing so. Or, supposing its action to be diminished, a weak action of the heart implies a diminished supply of the arterial influence. There can be no increased supply of the arterial influence, without an increased action of the heart; so that the heart must act more powerfully, in order to acquire the power of doing so. But this increased action of the heart is a debilitating process; is itself a source of exhaustion; so that the heart must exhaust itself still further, as the only means of acquiring power. Is it not manifest that there must be some *other* source of power? It has been supposed that, under these circumstances, the nervous influence may act as a stimulus to the heart; but increased action, the effect of a stimulus, is itself a source of exhaustion; so that the heart must still exhaust itself, as the only means of acquiring power. What is the simple explanation of the fact? the nervous influence is a secondary source of power, is a secondary source of the arterial influence. This unites with the heart, independently of any increased action of the heart itself, and thus imparts power, without the necessity of any debilitating action of that organ. In cases of suspended animation, we have recourse to the artificial supply of oxygen through the medium of the respiratory organs; in fainting, we apply a stimulus to the nervous system; and we succeed in restoring the action of the heart by these apparently different remedies. The heart itself, in both these cases, is in the same state. Its irritability must be restored. How do these different remedies produce the same effect? If the nervous and arterial influences be the same things, the question is easily answered. The artificial respiration, and the increased action of the nervous system, equally supply oxygen to the heart. The cause is the same, and the effect produced is also the same. The points of analogy between the arterial and the nervous influences may be arranged under the following heads:—

Oxygen is necessary to the excitement of both. They both influence living action. They both influence living action in the same manner.

There is reason to suppose that they change the colour of the blood in the same manner.

There is reason to suppose that they both contain electricity.

There are no facts which point out any dissimilarity between the two, considered either as to the nature of their constituent principles, or as to their agency in the animal system.

And lastly, the phenomena of living action seem to indicate the necessity of this secondary source of power. The effect being the same, we have a right to presume that the cause is also the same.

Many collateral arguments might be derived from the correspondence observable between the perfect or imperfect development of the respiratory organs, and the perfect or imperfect development of the nervous system, in various classes of animals; and by considering the advantages which the superior animals derive from this secondary source of power, and the peculiarities which distinguish those animals, in which this secondary source of power is less distinctly marked, or altogether deficient. But these facts will present themselves to our notice in the course of the following observations :—

I have already stated that the theory, which imagines the brain to be a large gland, communicating its secreted fluid to the several parts of the system, through the medium of its nerves, is untenable, because it is inconsistent with all analogy, and because it involves the physiological contradiction, tha the same organization has not the same function to perform. If the arterial and nervous influences be the same things, a secretory action of the brain and spinal marrow is wholly unnecessary. In the language of Mr. Burns, the brain takes "the vitality of the air" from the blood. According to Dr. Darwin, "a fluid, perhaps much more subtile than the electric aura, is *separated* from the blood by that organ, for the purposes of motion and sensation." According to Mr. Hunter, "*breathing* seems to render life to the blood, and the blood conveys it to *every* part of the body." The arterial influence, then, is sent to the brain in a greater proportion than the immediate necessities of that organ require, and is there accumulated for certain purposes; and the whole nervous system is simply to be regarded as a peculiar organization, capable of accumulating, and of transmitting to the several organs of the system, according to their necessities, the *arterial* influence. We thus see why the brain and nervous system should be supplied with a larger quantity of blood than is necessary to the support of their own individual structure and action. They are secondary sources of power, and transmit to the several organs that influence, which cannot be supposed to be imparted by the circulating system alone, without involving the contradiction, that an organ is capable of restoring its own power by the same action which had previously exhausted it, or the improbability that every increased action of the system is productive of disease, of an inflammatory diathesis, which threatens the integrity of the vital organization.

I have said that the arterial influence is communicated to the nervous system in a greater proportion than its immediate necessities require. The fact is virtually admitted, because this is the leading circumstance which has induced physiologists to attribute to the brain a secreting action; and yet sensation and motion *immediately* cease, upon the supply of the arterial influence being intercepted. How are these contradictory circumstances to be reconciled? The action of every organ in the body is dependent upon some influence derived from another. No organ can impart power to itself. Even the heart is not an exception to this rule. The heart, as the centre of circulation, supplies blood to itself; but the coronary arteries are the intermediate organs, which render that blood efficient. It is not the organ itself, but the assimilating action of the coronary arteries, which supports the structure of the heart. The heart, as the centre of circulation, must supply itself with blood, because the organization of the whole system is dependent upon that single source; but the *action* of an organ is not solely dependent upon the nervous influence. Even M. Le Gallois, who seems to regard the nervous influence as nothing less than the vital principle, does not imagine that the brain supplies to itself its own power of action. "C'est cette impression, c'est ce principe formé dans le cerveau et la moëlle epinière, qui sous le nom de puissance nerveuse, et par l'intermediaire des nerfs, anime *tout le reste* du corps." We all admit, that arteries are supplied with arteries, because the existence of the vital organization is wholly dependent upon a due supply of blood. We speak of the *vasa vasorum*, but no one imagines that the nerves are supplied with nerves; that the brain conveys brain to itself. "La vie est due à une impression du *sang arteriel* sur le cerveau." The action of the brain is admitted to be influenced by arterial blood alone "For the support and production of the nervous energy, a certain substance, namely, arterial blood, is requisite. This fluid, by its circulation, not only acts as a natural stimulus to the nerve, exciting and supporting its action, and thus enabling the energy to subsist; but it also affords the materials from which the energy is drawn."* We admit, then, that the action of the brain is *solely* dependent upon the arterial influence; and we cannot deny, that the action of muscular fibre is connected with the nervous influence. We *thus admit* the existence of two sources of power in the system: why should they be different? Arterial blood, if it acted as a stimulus only to the brain, might excite action; but action exhausts, instead of supporting the power of an organ. How then is the susceptibility of the brain maintained or restored? There is but one source—the arterial influence—from which it can be derived; and we must presume, that this influence supports the irritability of the brain and of the heart in the same manner. The observations of Dr. Reil seem to throw some light upon this subject. "The neurilema, according to our author, is not an accidental, but essential part of the nerve. It determines their consistence, form, and organization. It supplies the continual waste of medullary matter, which is topically secreted in every part of the nerves by the neurilema; and, therefore, the nervous power, depending on the quality and quantity of the medulla, is generated in the nerve itself, and does not proceed from the brain as was formerly imagined; every portion of nerve, even the smallest, lives of itself, is nourished, and increases independently, and, *by its own power, is irritable, and capable of performing its functions.*"* An accumulation of the arterial influence may exist in the nervous system; but this can only be rendered efficient by a certain action of the nerve itself, and that action is dependent upon the arterial system alone. I shall endeavour, in a future part of these observations, to show, that the action of nerves is similar to that of the arteries. I conceive, then, that the arterial influence is communicated to the brain and to the heart for precisely the same purpose, viz. that of supporting their irritability, that is, their susceptibility of action only. Thus the action of the brain may be wholly dependent upon a continual supply of the arterial influence, through the medium of the blood-vessels, although an accumulation of this principle may, at the same time, exist in the brain; because the nervous influence must be subjected to a living action of the system, before it can contribute to the irritability of an organ; and the nerves which communicate this influence to other organs, cannot, consistently with the general laws of the system, communicate this influence to themselves.

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* Burns.

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* Annals of Medicine, 1798.

I have said that the arterial influence is *accumulated* in the nervous system. This may be supposed to require some proof; but the same idea is expressed by physiologists in general. Dr. Darwin says "When we recollect that the electric fluid is actually *accumulated*, and given out voluntarily by the Torpedo and Gymnotus Electricus." "Mr. Hunter thought that a similar vital principle was diffused throughout the body in *different* degrees." Mr. Cuvier says, "by *restoring* to the fibre its *exhausted* irritability." If electricity, life, and irritability, exist in different degrees or portions at different times, there must, at one time, be a comparative *accumulation* of these several principles. If we admit that the arterial influence is capable of being accumulated in a muscle, why may it not also be accumulated in a nerve?

The idea, then, which I entertain of the nervous influence is this:—The air is a compound body, existing in a state of *chemical* union. This, according to the electro-chemical doctrine, irresistibly suggested by the recent experiments of Sir Humphrey Davy, and, I may say, generally adopted, implies, that the several principles of the air, not only contain electricity in themselves, but are kept in a state of chemical union by the equilibrium of electricity which is established between them. The air, upon its entrance into the lungs, is digested; that is, the several principles are disunited; oxygen, the nutrient principle, (if I may use the expression,) is converted into the arterial influence, and this bears no nearer relation to the air, from whence it is derived, than chyle to alimentary matter: nitrogen and carbonic acid, the excrementitious principles, are, like the fœculent parts of our food, at the first convenient opportunity, eliminated from the system. The dis-union of the several principles constituting the atmosphere, must, according to the present chemical theory, depend upon their equilibrium of electricity having been destroyed. Oxygen, then, containing a certain portion of electricity, unites with the blood; but, in consequence of its natural portion of electricity having been altered, (and this fact is implied by the decomposition of the air, because, without this change, the decomposition could never have taken place,) it no longer preserves those chemical properties which characterize oxygen in its natural state, and therefore it cannot be discovered in the blood by the usual chemical tests. Oxygen, in this peculiar state, constitutes the arterial influence, but when separated from the blood, and accumulated in the brain, spinal marrow, or nerves, is denominated the nervous influence. There is reason to suppose, that the brain does not elaborate from the blood a new substance, that is, any thing which did not previously exist in the blood itself, because we cannot attribute to the brain a secreting action (confining the term to the operation of the brain upon *the blood)* without infringing upon the fundamental and established law of physiology, that the action of an organ always corresponds with its organization. I presume, therefore, that the *superabundant* arterial influence, not that which is necessary to the action of the organ itself, is simply *separated* from the blood by the brain and nervous system. This idea seems to be expressed by Mr. Burns, when he says, "in certain diseases, the brain does not *take* from the blood as much vitality as it is capable of yielding."

I have assumed the identity of the nervous and arterial influences upon the following grounds. Oxygen is essential to the excitement of both. They both influence living action. They both influence living action in the same manner. They both change the dark hue of venous, into the scarlet colour of arterial blood. The analogy between electricity or galvanism, and the nervous and arterial influences, is equally strong. We cannot suppose them to be different, without attributing to the brain a secreting action which is inconsistent with its organization. We cannot suppose them to be different, without imagining that the arterial influence *supports* the action of one organ—the heart; and *exhausts* that of another—the brain. We cannot suppose them to be different, without involving the physiological improbability, that the same action in the same organ, is excited by different stimuli. We cannot explain the phenomena of living action, without admitting the agency of a secondary source of power; and, consequently, we cannot suppose the nervous and arterial influences to be distinct things, without attributing the same effect to different agents. And lastly, these difficulties are avoided, contradictory opinions are reconciled, the phenomena of living action are more simply, and, in my opinion, more satisfactorily explained, by our regarding the nervous and arterial influences as the same things.

I shall proceed in my endeavour to explain the phenomena of living action, both healthy and diseased, consistently with the physiological principles already suggested; but, before we enter upon this subject, a short review of these principles appears to be necessary. The action of every organ, considered individually, is dependent upon, and regulated by, the peculiar arrangement or proportions of its elementary particles. But the organization is itself dependent upon a series of actions, by which the food we take, and the air we inspire, are first converted into component parts of the animal frame, and then, having accomplished their destined functions, having been deprived of their vitality and become excrementitious, are, as soon as convenient, eliminated from it; so that the living action of a part, considered individually, is *immediately* dependent upon its organization, and *remotely*, upon the combined actions of the system. The organization of the system cannot support itself. The constant supply of food and of air, which we find to be essential to the continuance of living action, implies that a constant expenditure is going on. The food and the air, in their excrementitious state, that is, when deprived of the vitality which they had acquired through the medium of the assimilating functions, are thrown off by the different excreting organs. Thus, in the carbonic acid perspired and expired, and in the excretions generally, we recognize the refuse, if I may use the expression, of the food and air, which had acquired vitality through the medium of the assimilating organs; and which, having accomplished their functions and lost their vitality, had become the legitimate objects of lymphatic absorption. We find that the supply of food and of air is proportioned to the action of the system; it is, therefore, reasonable to suppose, that the expenditure is produced by the same cause, which renders the supply necessary. It appears, then, that the action of the system is both a source of exhaustion and of supply. How is this fact to be reconciled with probability? The action of every *individual* part produces that change in its organization, by which a certain proportion of its elementary particles lose their vitality, and become excrementitious. The action of a part cannot, at the same time, exhaust and restore; but the exhaustion is dependent upon the immediate action of the organ itself, the supply upon that of other organs. It would appear, then, that if no action took place, no supply would be necessary, because no change in the organization would occur. Do we not find, that this is actually the case in the seed, in the egg, and in torpid animals, all of which, *if no action be excited*, will retain their vitality for a number of years. An imperfect action, that is, an action where the expenditure exceeds the supply, will produce that change in the organization, which is inconsistent with life. Thus, by excluding the air from an egg, we prevent *all* action, and may be said, by artificial means, to render the animal torpid. But if the egg be exposed to the air, an imperfect action—an action, for instance, which is of itself unequal to the support of its natural temperature—will take place, and the particles will undergo that change which is incompatible with the continuance of life. The peculiarity, then, of torpid animals, seems to depend upon that law of nature, by which *all* action ceases, before the organization has undergone a change, which is inconsistent with its vitality. Living action is dependent upon the supply both of food and of air; but it seems to be more immediately influenced by the latter, and this we might *a priori* expect, because a *constant* supply of air, and only an *occasional* supply of food, are found to be necessary. Our observations, therefore, upon the nature of living action, will be more particularly directed to the influence of the air upon the system. We have given our reasons for supposing, that the oxygen of the air is a component part of the arterial *and* nervous influence, and therefore it will be found, that we have extended the influence of the air beyond that sphere which has usually been allotted to it. Were we to disregard the change which the air undergoes in its passage through the lungs, we should say that the nervous influence is nothing more than the air, accumulated in certain organs, for the purpose of answering any increased demand which any increased action of a part might require, and which demand cannot be supplied by the circulating system alone, without an irregular or increased action of the heart and arteries, which is inconsistent with the welfare of the system; or, without calling upon the heart and arteries to supply power at a time when they are themselves exhausted and unequal to the task. It is necessary for the welfare of the system, that the action of the heart and arteries should be independent of the will. How could this in reality be the case, if *every* voluntary action of the system required an increased action of the heart and arteries to support it? How has nature guarded against the inconvenience which would necessarily arise from such a state of the system? The action of the voluntary muscles in the superior animals (in which the operation of the will is more irregular, and this provision of nature consequently more necessary) is wholly dependent upon the nervous influence. This secondary source of power has been placed, as it were, under the command of the animal; but as soon as the nervous influence is reduced below the standard which is consistent with health, that is, as soon as the phlogistic diathesis commences, fatigue is experienced, and the power of the animal to exhaust still further the nervous influence, is over-ruled by the disinclination to exert it. The violence of a madman is not attended with that sympathetic action of the system which characterizes the exertions of another individual. There is a greater accumulation of the nervous influence, and, consequently, that increased action of the arterial system which constitutes the phlogistic diathesis, and which we have stated not to commence until the nervous influence is reduced below the standard consistent with health, does not take place so readily.* I have already given my reasons for supposing that the oxygen of the air undergoes a certain change in the lungs, by which it approximates to the galvanic influence, and that this may be a peculiar combination of oxygen and electricity. It remains for me to point out in what manner this peculiar combination, existing in a state of union with the blood, or accumulated in the nervous system, may produce the various phenomena which characterize living action. All living action is motion, but the peculiarity of living motion is this—that it not only produces a mechanical effect upon moveable solids, but a mechanical and chemical effect upon the fluids of the body, changing, not only their situation, but their properties; it produces also sensation and animal heat. The different actions of the system I shall arrange under the titles of *locomotive*, *assimilative*, and *sensitive* functions. But, before we can inquire into the nature of these functions individually, it is necessary to make some few observations upon that property of matter which we call life; to investigate the causes of living action generally, and of animal heat, which I consider to be intimately connected with the general functions of the system.

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* I have great reason to suppose, that affections of the mind are more influenced by the state of the blood than is generally imagined; I allude more particularly to that state of the blood, which is connected with respiration. If the nervous and arterial influences be the same things; if they are both to be regarded as certain modifications of the air; if the mind be influenced by the state of the nervous system, (a fact which is proved in syncope, apoplexy, and particularly by the effects of inspiring the nitrous oxide), may we not hope to influence the action of the brain through the medium of the respiratory organs, and thus to acquire some power over affections of the mind itself? May we not hope to account rationally, for the influence of the air upon a variety of diseases?

CHAPTER XII.

LIVING PRINCIPLE.

LIFE is a certain power, property, or principle, upon which those phenomena, which distinguish animate from inanimate bodies, are dependent. There are a certain set of philosophers, who vainly imagine that there is no limit to the human mind, that there is no difficulty which may not be overcome by the energies of the human understanding. But these philosophers have either never read Locke's treatise upon this subject, or, having read it, have not derived that advantage from the perusal which it is calculated to impart. Feeling (as I do most sensibly,) the imperfections of the human mind, I have no hesitation in using the words power, property, or principle, although they are, in reality, tacit confessions of my ignorance. If I am asked what life is, my answer is very short; I know nothing about it, and I never met with an author who gave me any information upon the subject. But if I am asked what are the phenomena which indicate the presence and agency of that unknown principle, denominated Life, I should answer there is but one—the preservation of animal or vegetable matter, (and perhaps I might add mineral,) under circumstances, which but for the agency of this principle, would destroy it. Mr. Burns says, " But by a more accurate examination, we shall find that there is, in reality, no individual whatever in any of the kingdoms or classes of nature, which can be called inert or truly dead; for, from the largest masses of matter, down to the most minute corpuscles, we distinctly perceive the operation of an active and immaterial principle. The particular nature of this principle, as we shall afterwards see, never *can* be ascertained. We are, however, sufficiently assured, by the phenomena which we observe, that in *all* the classes of nature such a principle does exist."* Dr. Paris says, " If life be defined that power by which organized beings are enabled to resist the physical and chemical operation of surrounding agents, it follows that death must be marked by the occurrence of those phenomena to which the elective attractions, no longer suspended or controlled, will necessarily give rise."† Does this definition include all the phenomena which are to be attributed to the agency of the living principle? I believe that it does. Does not living action indicate the presence and agency of the vital principle? It indicates the presence, but not the agency, of that principle. Does not the vital principle influence living action? I believe not; because a *similar* principle appears to exist in mineral matter which has no sensible action; because parts separated from one body (as in the experiments of Mr. Hunter) lose, in their state of separation, the power of being excited into action by their appropriate stimuli, but do not lose their vitality, as is proved by the circumstance of their uniting with other bodies, and preserving their own peculiar organization; because a paralytic limb is deprived of all sensation and motion, but retains its vitality; and because, in cases of apparent death, that is, when there is no indication of existing action, life still exists, as is often proved by subsequent recovery. It is very easy to say, that action still exists, although we cannot discover it; that a paralytic limb, and the detached comb of a cock, would be excited into action by their appropriate stimuli, if they could be applied; but as far as the facts go, as far as we are able to determine by our senses, (the only sources of information,) living *action* has positively ceased in apparent death, and a paralytic limb, and the detached comb of a cock, have lost the power of being excited into action by their appropriate stimuli. It cannot be denied that there is an intimate connection between life and action; where action is, there life is always present; but the reverse is not so clearly established, because life is often present where action is not—where, at least, we have no evidence of the existence of action. How then shall we distinguish between the presence and the agency of the vital principle, as far as living *action* is concerned? The union of life with matter is dependent upon a certain organization, and the agency of that principle upon matter is to prevent its decomposition. The irritability of an organ, that is, its power of being excited into action by its appropriate stimulus, is also dependent upon a peculiar organization; but the peculiar organization, which is necessary to the existence of that property of an organ which we denominate its susceptibility of action, and that peculiar organization which is essential to the union of life with matter, are different. For instance, the nervous influence may be cut off from an organ, and such a change may be produced in its organization, as shall render it incapable either of sensation or motion; but the change produced, though incompatible with the existence of that property denominated irritability, is not incompatible with the continuance of life. But if a still further change be produced, such a change as is incompatible with the continuance of life, the irritability of an organ is, of necessity, lost; because the *action* of an organ requires a more perfect organization than its life action; therefore, must always indicate the presence of life, because it indicates a more perfect organization than is necessary to the union of life with matter; but life does not always indicate the power of living action, because the organization, compatible with the continuance of life, is too imperfect for the continuance of living action. It seems to be a wise provision of nature, that the power of action should be lost before the existence of the vital principle is threatened. Let us suppose it to be otherwise; the first indication of fatigue would be death. I shall endeavour to illustrate my opinions upon this subject, by considering, more fully, the sentiments which have been expressed by other writers.

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* On Inflammation. † Med. Jurisprudence.

Life is a certain power, property, or principle, upon which the preservation of organized matter, surrounded by agents which have a tendency to decompose it, is dependent; but life and living action are not the same things. What should we think of that philosopher, who, upon seeing an apple fall to the ground, should denominate the phenomenon he observes, the principle of gravitation? should we not say, that he was unable to distinguish between cause and effect; and that he had mistaken the effect produced for the cause which produced it? And yet, in our reasonings upon the phenomena of life, upon a subject which, from its acknowledged obscurity, seems, in a peculiar manner, to require a clear and intelligible definition of that which is to be explained: this want of distinction between the effect produced, and the cause which produces it, is glaringly apparent. Haller says, "Non potest tradi vitæ definitio ante enarratam physiologiam; est enim, summa earum actionum, quas singulatim describit physiologia."* This definition has been adopted by many writers, but more particularly by Xavier Bichât, who considers life to be the *tout ensemble* of the functions appertaining to animal matter. Muscular motion is one of those functions which physiologists are called upon to describe; but muscular motion is not life. Sensation is another function; but neither sensation, nor sensation and muscular motion combined, constitute life. They are the effects of certain stimuli acting upon organized matter, which was *previously* endowed with the living principle. Without the *pre-existence* of this principle, those functions, which are supposed to constitute life, could never have been called into action. Dead animal matter never acquires life by its own action. That peculiar organization, which is essential to the union of life with matter, never occurs, except as the consequence of a *living* action. Life, therefore, always exists before function; and this coincides with the opinion already expressed, that life is compatible with an organization too imperfect for the existence of function. The united functions have no greater claim to be regarded as the life of the system, because, without the *pre-existence* of the vital principle, these actions could never have occurred. M. Le Gallois says, "La vie est due à une impression du sang arteriel sur le cerveau et la moëlle epinière, ou à une principe resultant de cette impression." The stimulus of arterial blood upon a *dead* brain will never produce life. Life must exist in the brain *before* the stimulus of arterial blood can produce the principle which that author denominates life. "C'est cette impression, c'est ce principe formè dans le cerveau et la moëlle epinière, qui, sous le nom de puissance nerveuse, et par l'intermediaire des nerfs, *anime* tout le reste du corps, et preside à toutes ses functions."* What claim has the nervous influence to be regarded as the life of the system? I have already shewn that the existence of the nervous influence necessarily implies the *pre-existence* of the vital principle. It has been said that the vital principle regulates and presides over the different functions of the body. This opinion was strongly inculcated by Mr. John Hunter, and is enforced, in rather a peremptory manner, by Mr. Abernethy. Respecting, as I do most highly, the abilities, the moral and professional character of this author, and acknowledging the valuable and highly important information which I have derived from his works, I trust, that although upon some occasions I have ventured to differ from him, my pen has been solely directed by that insulated feeling which arises from the desire of promoting the cause of science, and by that independence of thought, which can alone render an opinion valuable, and which so eminently distinguish the writings of this author. Mr. Hunter supposed the vital principle to be something superadded to organization, and it appears to me that no one who believes in the authenticity of the inspired writings, can entertain any other opinion. "God breathed into his nostrils the breath of life, and man became a living soul." Mr. Hunter did not consider that the nervous influence and the vital principle were the same things, but he thought that the vital principle regulated the actions of the system in a similar manner. "He thought that the vital principle was particularly accumulated in the brain, in those animals who possessed such an organ, and that it seemed in them a kind of source of vital energies, and that the nerves were internunciate cords, by which the affections of life were reciprocally communicated."* It is not easy to distinguish between the effects attributed by Mr. Hunter to the agency of the vital principle, and those attributed by authors in general to the agency of the nervous influence. The vital principle is imagined to be accumulated in the brain, to be distributed through the medium of the nerves, and to influence living action. Life is supported in acephalous monsters, in animals who have no brain naturally, and in animals whose brain and spinal marrow have been artificially destroyed. These facts are sufficient to throw some doubts upon the theory of Mr. Hunter. But I conceive the error of Mr. Hunter's theory, to have originated in a want of distinction between the vital energy and the vital principle of an organ. The vital energy of an organ, that is, its susceptibility of action, is constantly varying, and is the property of living organized matter. I have already stated that the irritability requires a more perfect organization than the vitality of an organ, and therefore the irritability of an organ always implies the co-existence of the vital principle, because it implies a *more* perfect organization than is essential to the union of life with matter. If this matter vary in its organization, in consequence of an increased or diminished supply of the nervous influence, the susceptibility of action will vary also; but this does not depend upon any variation in the quantum of the living principle, because an organ may lose its susceptibility of action, and still retain the characteristic property of life undiminished. It still retains the power of resisting the decomposing action of surrounding agents. We know no medium between positive life and absolute death; but we all admit the difference between the increased and diminished irritability of an organ. "Mr. Hunter told us that life was a great chemist, and even in a seemingly quiescent state, had the power of resisting the operation of external chemical agency, and thereby preventing the decomposition of those bodies in which it resided."* This is the acknowledged, and I believe the *sole* effect of life, as far as the corporeal part of the animal frame is concerned. The property of life is unconnected with any action of matter itself; that is, the vital principle is something superadded to organization, is wholly independent of organization for its *existence*, but when united with matter, has the power, to a certain extent, of preventing its decomposition. "Mr. Hunter shewed us that this chemist—life—had the power of regulating the temperature of the substances in which it resides;" but the subsequent passage shews clearly that life has not this power. "Seeds possessing the principle of life, *being placed within the influence of the atmosphere, and in contact with moisture*, produce heat, form sugar to edulcorate the first nutriment of the young plant, and, in short, bring about all those phenomena which characterize incipient vegetation."† Life may be essential to the occurrence of those actions by which animal

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* Prælectiones.

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* Principe de la Vie.

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* Abernethy's Physiological Lectures.

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[*] Abernethy's Physiological Lectures. [†] Ibidem.

heat is produced, but the influence of the atmosphere and of moisture is equally essential. A cord will not vibrate except it be strained between two given points ; it is, metaphorically speaking, dead, inasmuch as it does not possess the capacity of being excited into that peculiar action which constitutes vibration. Animal matter, deprived of the vital principle, is in precisely an analagous state. This cord, when placed in a certain state of tension, has acquired the *capability* of being excited into a vibratory action. Matter, endowed with the principle of life, has acquired the *capability* of being excited into action. If to a cord, in a certain state of tension, a certain degree of impulse be applied, vibration is the consequence. If to living organized matter an appropriate stimulus be applied, action is the consequence. The tension of the cord is *necessary* to the vibratory action, but the impulse applied is equally necessary. We do not attribute the vibration excited, either to the tension of the cord, or to the impulse applied, individually, but to the concurrent agency of both. Thus living organized matter can no more *of itself* produce heat, than tension can produce vibration. "He further shewed us, that life, by its chemical processes, could convert a great variety of dissimilar substances into one kind of generally distributed nutriment, and could also prepare from it a still greater variety of dissimilar substances."* Life cannot, of itself, produce a secreting action of the arteries: the stimulus of the blood is equally essential to the existence of that action. "Mr. Hunter shewed us, that life was also an architect; that it built up a great variety of curious structures or edifices, in which it continued to reside and execute its future functions."† No person can be said to preside over an action which he cannot regulate at will. If there are concurrent circumstances, over which that individual can exercise no control, the effect produced cannot be attributed to the will of the individual. If, for instance, I take up a violin and execute a piece of music, that instrument is clearly the agent of my will, because, the instrument being perfect, I have a direct control over all the circumstances which are necessary to the production of sound, as far as the agency of the instrument is concerned. But if I place an Æolian harp in a current of air, the sound will vary, not according to my will, but according to the variable impulse of the air, over which I can exercise no control. To imagine that I presided over the action of the harp, would be as ludicrous as the idea of the organ-blower, who fancied that he was discoursing sweet music. If life cannot produce vision without the impulse of the rays of light, how can life be said to preside over that action of the retina which constitutes vision, when the action is equally dependent upon the *agency* of light, over which the vital principle can exercise no control? Mr. Abernethy says, "No unprejudiced person can, I think, possibly doubt, that it is by the active powers of life such effects are produced."* Having expressed my doubts upon this subject, and my reasons for entertaining them, I will venture to say that life is only a concurrent circumstance in that action of matter, which has been *termed* living action. I have already said, that a cord in a state of tension, and animal matter possessing the principle of life, are in precisely analagous states; that a cord, when placed in a certain state of tension, has acquired the *capability* of being excited into a vibratory action; and that animal matter, possessing the vital principle, has acquired the *capability* of being excited into animal action. But does it follow, that the capability of action in these two cases is owing to tension in the one, and life in the other? If tension, a power by which the particles of matter are kept at a certain distance from each other, prevail over that power by which these particles are brought into a nearer state of approximation, no vibration will ever take place. The capability of vibratory action does not correspond with the degree of tension, because, if this be *increased* beyond a certain point, the capability of action is not only not increased, but is positively counteracted. How can we attribute the capability of a certain action to a power which actually destroys it? The susceptibility of action in animal matter is constantly varying, but its life is, as far as we can ascertain by our senses, (the only sources of information,) always the same. For instance, a person may sleep with his eyes open, and the rays of light may be impinged upon the retina. The retina, the brain, the organs of vision generally, possess as much *life* in a sleeping as in a waking state, but they clearly do not possess the same susceptibility of action, for the stimulus of light will not produce that action which constitutes vision. Hybernating animals, in their active and inactive state, are equally endowed with life; but no one imagines that their susceptibilityof action, in these two states, is the same.

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* Abernethy's Physiological Lectures. * Ibidem.

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* Abernethy's Physiological Lectures.

"It is a well-known fact, that dried seeds, *although exposed to heat and air*, may be kept for a great length of time without undergoing any sensible change; nor do they, in that state, as we found by experiment, in the least degree affect the air in contact with them."* Now it must be admitted, that life resides in the seed; for this is proved by the subsequent germination, and by the organization of the seed remaining unaltered, although surrounded by chemical agents which have a tendency to decompose it; but the life of the seed does not impart even that *susceptibility of action* which may render it sensible to the stimulus of heat, light, or air."† If, however, *moisture* have access to them, they presently begin to swell, which is the first step towards their evolution." Moisture then has imparted a property which life could not, and that property is only a susceptibility of that action which we denominate vegetation; because, without the contact of the air, or some influence derived from the atmosphere, that action will not take place. This fact is proved by the circumstance, that seeds, buried at a certain distance beneath the surface of the earth, may possess both life and moisture, but will not vegetate. We have here a positive proof,

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[*] Ellis's Inquiry. [†] Ibidem.

that as far as seeds are concerned, not only life and living action, but life and the susceptibility of action are distinct things." "Snails in their shells have been thrown into a drawer, and lain by for fifteen years; but recovered action on being immersed in a basin of water. The Gordius, or horse-hair eel, while in water, is in incessant motion; but, if the water dry up, its movements cease, it shrivels up, and may be kept in this state for an indefinite length of time; but place it again in *water*, it begins to move, and in a few minutes is as brisk and lively as ever. There is an animalcule which sometimes resides in wheat, which, after lying dormant for nearly thirty years, has recovered its vital functions, merely by *moistening* the grain in water. The Rotifer (vorticella rotatoria,) which lives in small puddles of water, often on the tops of houses, shrivels up as the water evaporates, till it becomes like a piece of dried parchment, in which state it may be preserved for years without suffering the smallest change, but on moistening it with water, it resumes its pristine form, and soon becomes as lively as ever. Suffer the water again to evaporate, and the animal dries up as before; but restore to it the moisture, and again it is brought to life. In this way it has been alternately deadened and revived eleven times without any apparent diminution of its vital powers; and although subjected, during its torpid state, to a heat of 56° Reamur, and a cold of 19, it was equally susceptible of revivification as at first. These facts sufficiently demonstrate the necessity of water to the commencement of animal *action*."* But the seed, the snail, the Gordius, the animalcule, the Vorticella Rotatoria, all possessed *life* without water. We have no reason to suppose that they possessed more *life* in their active, than in their inactive state; because the peculiar property of life, that of preserving the integrity of their organization under circumstances which, but for the agency of that principle, would destroy it, is equally apparent in their quiescent and in their active states. Moisture, then, in these animals, is essential to the existence of animal *action*, but it is *not* essential to the agency of the vital principle. Life, therefore, and living action, are clearly not the same things, and ought not to be confounded together; but this distinction is but little regarded. In the passage already quoted, the author says, " place it again in water, it begins to move, and, in a few minutes is as brisk and *lively* as ever;" and again,

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* Ellis's Inquiry.

" but on moistening it with water, it resumes its pristine form, and becomes as *lively* as ever." Now, in point of fact, they were as lively, that is, they contained as much life in their dried as in their moistened state. But in their dried state, vegetable and animal matter, although they possess life, do not possess irritability or *susceptibility* of action; because those substances—light, heat, and air—which will produce action in vegetable and animal matter, *when moistened*, will *not* produce action in vegetable and animal matter when dried, although in their dry state they evidently possess life. Life, therefore, and *susceptibility* of action, are not the same things. Neither can life impart to matter its *susceptibility* of action, because life, as we have already seen, exists in matter which is not sensible to its appropriate stimulus; but if moisture be added, by which life is not at all increased, matter acquires that property, by which it becomes capable of being excited into action. We thus see why, in hybernating animals, life may exist without action;—why, in cases of apparent death, no action may exist, although life be not extinct;—why a person may be buried beneath a super-incumbent weight of snow, of earth, or of water, may be actually cut off from all communication with the atmosphere, and retain, for a certain time, his vitality;—why persons have been buried alive, and why putrefaction has been considered by many authors as the only certain sign of dissolution. In cases of this kind we may believe our senses, we may admit that all living *action* has ceased; but we must at the same time remember, that life and living action are two distinct things.

But living action always implies the presence of life; how shall we distinguish between the presence and supposed agency of this principle? We have already seen that life may exist in animal matter deprived of its moisture, but the susceptibility of *action* requires the *additional* influence of this property. Life, therefore, is compatible with an imperfect organization, compared with that which is necessary to living action; but the more perfect organization, which is compatible with living action, implies the less perfect organization which is compatible with life; and therefore, where action is, there life must be always present; but the presence of life, being compatible with a more imperfect organization than living action, does not always imply the presence of that more perfect organization, which is necessary to function.

Life, therefore, as far as living *action* is concerned, may be considered as only a concurrent circumstance, which is not at all concerned in the action itself, but which is always present; and life may be defined to be that principle which preserves the organization of matter under circumstances which, but for the agency of that principle, would lead to its dissolution. I have the authority of Mr. Hunter for supposing, and that of the sacred and inspired writings for asserting, that life is something superadded to organization. "God breathed into his nostrils the breath of life." I cannot for a moment admit, that I am called upon to give an allegorical interpretation to a passage, the clear, simple, and literal meaning of which, is so evident, so consistent with the true spirit of Christianity, so accordant with the doctrine of our revealed religion, and so intimately connected with the most important interests of mankind. Life is something superadded to organization ; it is wholly independent of matter for its *existence*, and therefore it may be separated from matter and still exist. Matter may be perishable, but life is immortal. But the *union* of life with matter may be, and appears to be, dependent upon a certain organization. The great object, then, of medical practice, is to preserve that integrity of organization, which is compatible with this union between life and matter. But as living action is the only criterion by which we can form any estimate of the perfect or imperfect organization of matter, our attention, except in cases of apparent death, is necessarily directed to that state of the organization, which is compatible with living action; and that, as has been already shewn, includes the more imperfect organization, which is necessary to the union of life with matter. Life, then, is a certain principle superadded to organization, wholly independent of organization for its *existence*, but altogether dependent upon organization for its *union* with matter; and its sole agency upon the *corporeal* parts of the system, is to prevent, as far as is consistent with the natural life of each individual, the decomposition of matter under circumstances, which, but for the agency of this principle, would destroy it.

Physiologists, however, have not been contented with this simple view of the subject. Life has been supposed to be the effect of matter fortuitously arranged, so as to produce, by an internal action, that effect which we denominate life; it has been supposed to be the effect of matter purposely arranged, so as to produce that effect; to be the nervous influence, to be motion, to be electricity, galvanism, gravitation, attraction, or indeed any principle, the agency of which we witness, but the nature of which we are wholly unacquainted with.

That life is the effect of matter, *fortuitously* arranged, is not only in direct opposition to the evidence of Scripture, but to reason and common sense; and I recommend to those, who, *in reality*, harbour this opinion, (for I very much question the *honesty* of writers upon this subject) a perusal of Dr. Paley's "Natural Theology."

That matter *might* have been so arranged, as to produce life as an effect, it is impossible to deny. But we find that matter never is so arranged, except through the medium of *living* matter; and it is absurd to say that a thing *was* done, for no other reason than because it *might* have been done.

The idea that life and the nervous influence are the same things, may easily be traced to a want of discrimination between life and living action. The nervous influence supplying a muscle may be interrupted, and that organ may become paralytic, but it possesses as much life as it did before. In opthalmia, the eye is often so sensible, that it will not bear the stimulus of light. This increased sensibility of the retina is attributed to increased action of the optic nerve, and if to increased action of the nerve, to an increased supply of the nervous influence. Can we for a moment suppose, that the retina, in a state of disease, possesses more life than in its natural and healthy condition? The sensibility of an organ, which property is attributed to the nervous influence, may vary from its maximum in inflammation to its minimum in paralysis, and its life still continue the same.

To say that life is motion, without explaining the peculiarity of that motion, which can produce the phenomenon in question, is to say nothing. Does the following passage give us any information upon this subject?—"The whole of nature may be supposed to consist of two essences or substances, one of which may be termed spirit, and the other matter. The former of these possesses the power to commence or produce motion, and the latter to receive or communicate it. So that motion, considered as a cause, immediately precedes every effect, and considered as an effect, immediately succeeds every cause."* We all admit (for I believe that those who deny the fact admit it in their consciences) that there is a superior power which regulates and watches over the actions of the universe; this power we may denominate spirit; but the word conveys no definite idea of the attributes of that power, because it is impossible that language should convey an idea, which the human mind is incapable of suggesting. But the word spirit conveys the idea of something which is divested of the *limited* powers and *perishable* nature of matter; it conveys an idea of something superior to matter, and therefore it conveys an idea of the unlimited powers and the imperishable nature of that first cause of all, which rules the universe. But to say that spirit is motion which produces motion, and that that motion is life, is not only useless but pernicious. It is useless, because it leaves the thing to be explained just where it was before, with the additional obscurity of an imagined agent, which defies all explanation. What is that motion which produces motion, or how does that motion, which produces motion, prevent the decomposition of animal matter, under circumstances which, but for the agency of this peculiar kind of motion, would decompose it? It is like endeavouring to ascertain the colours of a picture, by allowing the rays of light to pass through a piece of smoked glass. It is pernicious, because it *may* convey the absurd idea (I am very far from intimating that such was the opinion of the author) that that motion which produces motion, was itself set into motion by its own inherent power, and not by the agency of a superior will.

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* Darwin's Zoonomia.

To imagine, as some authors have done, that life is motion, produced by the *fortuitous* arrangement of the particles of matter, is to suggest an idea, which nature in all her works and with a voice not to be stifled, most decidedly contradicts; and to imagine that life is motion, produced by a peculiar and purposed arrangement of the particles of matter, is to suggest an idea, which only adds obscurity to what was sufficiently obscure before. There is as much reason to say that motion produces, as that it prevents, the decomposition of matter.

To say that life is electricity or galvanism, involves the contradiction, that an accumulation of life will destroy life; and I do not agree with those authors, who believe that there is a resemblance between the two. Mr. Abernethy says, " Now there is no analogy between the permanent and invariable properties of gravitation and elasticity, and the occasional and variable properties of life. Therefore, if I judge from analogy, I must think, as I have hitherto done, that life is more like electricity or magnetism, because its operations are occasional; it may vary in degree, and admits of being annulled or abstracted, without evident difference in the subject to which it has belonged."* Believing, as I do, that the property of life is uniformly and universally the same, and that the phenomena of living *action* may be explained without the agency of the vital principle at all, the arguments adduced have no weight in convincing me, that life is *more like* electricity than gravitation or magnetism. Life may preserve that integrity of organization which is essential to its union with matter, but life does not preserve that integrity of organization which is essential to living action, because we find that life exists in matter which does not possess the *capability* of action, and because we find that the susceptibility of action is acquired through some other medium than life. It will be said that this medium, moisture for instance, cannot be assimilated with the system, so as to become the source of living action, without the assistance of the living organization. Granted. But this does not prove that the assimilation is owing to the agency of the vital principle connected with organization; it may be owing to the agency of the organization in which the vital principle is resident, but exercises no further influence over living action, than preserving the organization of matter which is essential to it; which influence was equally exerted before the action took place. But this influence does not extend to that organization which is essential to living action, because we find that a part may *lose* the power of action, although the vital principle still continues to reside in it. The organization of a combustible body, a piece of coal for instance, is dependent upon a certain property, by which the particles of matter are kept in that state of union which constitutes organization. This property has been denominated the attraction of cohesion. If to a combustible body, thus organized, heat and oxygen be added, that action termed combustion commences. No one attributes this action to the agency of that power, the attraction of cohesion, which kept the particles of the coal in a state of union; and yet, without that power, the particles of matter would have been dissipated, and combustion would never have taken place. Life, in my opinion, has no more to do with living action, than the attraction of cohesion with combustion. They are both concurrent circumstances which must be always present, but do not immediately influence the action which takes place. How life can be said "to vary in degree, and to admit of being annulled or abstracted without evident difference in the subject to which it belonged," I cannot well conceive, when I find that the decomposition of matter is the effect of life being annulled or abstracted. To say that life is gravitation or attraction, is, in reality, saying that we know nothing about it; but the *invariable* property of life would lead me to judge, by analogy, that it is more like gravitation or attraction than electricity.

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* Physiological Lectures.

The property of life is to preserve the organization of matter, and therefore Mr. Hunter suggested that living matter is never decomposed by the action of the stomach, as long as it retains the vital principle. In what part of the system does our food acquire animal life? I am not disposed to discuss, at any length, the question of the vitality of the blood, but I may observe that there is a marked inconsistency in the idea suggested by Mr. Hunter, that the blood is alive. He tells us, that living animal matter is undigested by the action of the stomach, in consequence of its vitality which resists decomposition; and yet the blood is formed for the very purpose of being decomposed. What possible advantage could the system derive from the vitality of the blood, from the blood being endowed with the property of resisting that very action, which it is the object of nature to accomplish. "Mr. Hunter wonders that this opinion had not been more deeply impressed on the minds of medical inquirers, because the blood undergoes *evident changes* from variations in the state of the health, and the actions of the vessels." Is not this, consistently with Mr. Hunter's own opinions, the strongest possible argument against the theory? The following argument is equally unsatisfactory and inconclusive. "Life is generally attributed to solids only, yet these are formed from the blood, *which could not give them what it did not possess.*"* Whence, then, does the blood derive its vitality? The food, having been deprived of the vital principle, "could not give what it did not possess." If the living action of the stomach be capable of imparting life to aliment, which was deprived of its vitality, why may not the living action of the nutrient arteries be capable of imparting life to that, which is derived from a dead fluid? Professor Cuvier says, and apparently with truth, "tout le corps est nourri par une fluide *stagnant.*" The coagulation of the blood cannot be regarded as a proof of its vitality, because that change is more apparent in its diseased, than in its healthy state; because it never occurs, as long as that fluid is contained in the circulating vessels, its natural situation; and because, if it did occur, the general *death* of the system would be the consequence. The coagulation of the blood cannot be a proof of its life, because, without fluidity, the blood must be considered as an useless mass. We are told by Mr. Hunter, that the blood is kept in a fluid state by the living action of the arteries, which amounts to this; that the blood is endowed with the power of coagulating into a mass, for no other ostensible purpose, than that of requiring an action of the arteries to counteract it. I cannot say that Mr. Hunter has advanced any arguments which convince me of the vitality of the blood, but it is a question of no practical consequence, and appears to have been kept alive, solely, by the distinguished character of the author who first suggested it. Were I to offer an opinion upon the question proposed, viz. in what part of the system the food first acquires its vitality, I should say, that the object of our taking food, is its conversion into living animal matter, that is, into the solids of the body; that the peculiar property of life is to prevent the decomposition of matter, and, therefore, I cannot suppose, that any substances which are intended to be decomposed by the actions of the system previous to their constituting parts of the living frame, can be endowed with a property, the acknowledged agency of which is to *prevent* decomposition. Food, therefore, cannot be supposed to have acquired vitality before it has undergone the *ultimate* change, by which the object of its being taken into the system is accomplished. I believe that all solids of the body are endowed with the living principle; that the spiritual part, whatever it may be, is eminently endowed with this principle, and is immutable and imperishable; and that the question as to the vitality or non-vitality of the fluids is more speculative than useful, and might as well be consigned "to the tomb of the Capulets." I shall conclude this part of my subject exactly where I began, as to the nature of the vital principle. "Of the ultimate nature of the vital principle which operates in organized beings, much has been said or written, but after all we are entirely ignorant of it, and shall ever remain so; for as we only distinguish things by comparing together their qualities or their quantities, it is evident we can never arrive at any knowledge of a principle which possesses neither of these requisites, and which moreover produces effects *sui generis.* But although we cannot arrive at a knowledge of the vital principle itself, we are enabled, from the nature of the means it employs and its operations in general, to form a tolerably correct idea of the extent of its capabilities, and to say, what it can, and what it cannot, do, or rather what it does, and what it does not, accomplish."* With respect to the *agency* of the vital principle, the only function of life, if I may use the expression, is to preserve the organization of the system under circumstances which, but for the agency of this principle, would destroy it. "Life is a principle which we can only detect and judge of by its operations or actions; and when these are not exhibited, we are apt, *though sometimes erroneously*, to conclude that the body is dead. The leading property of life is to communicate a preservative power to every individual with which it is connected. This is sometimes effected by very evident and intricate actions; but at other times it is exerted without any sensible operation. The crystal resists, to a certain degree, mechanical impressions, which would destroy its form. The blood, when newly drawn, the sap of vegetables, the living egg, resist cold, by an imperceptible operation, to a much greater degree when alive, than after their peculiar life departs. They resist all the efforts of chemical agents which act on common matter; nor is it *possible* to decompose or injure them in this way, until they lose their specific life, and descend in the scale of existence. This *simple preservative power* is a discriminating mark of the presence of life; but we cannot detect it until we apply destroying causes; it is the uniform and universal effect of the *combination* of life with any substance, whatever its nature or structure may be; it is *the essential characteristic* of life, which it must shew whenever it is present."*

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* Physiological Lectures.

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* Prout on the blood.

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* Burns.

CHAPTER XIII.

Living Action.

ALL Living *Action* is motion, but living action implies the presence not the agency of the vital principle. I may appear, to some of my readers, to have taken a great deal of unnecessary trouble, in my endeavour to distinguish between the life and the action of an organ; but how could we "form a tolerably correct idea of the extent of its capabilities, and say what the vital principle can, and what it cannot, do, or rather, what it does, and what it does not, accomplish," without inquiring "into the nature of the means it employs, and its operations in general." The distinction between the presence and the agency of the vital principle, which I have *endeavoured* to establish, must, I think, if at all, be wilfully mistaken. The particles of water are kept in a state of union by a certain power called attraction. The particles of animal matter are kept in a state of union by a certain power called life. If to the particles of water in this state of union, caloric be added, they are converted into steam, that is, the relative situation of the particles is altered; if to the particles of animal matter, in this state of union, an appropriate stimulus be applied, action is the consequence, that is, the relative situation of the particles is altered. If, in the former instance, we do *not* attribute this change to the agency of attraction, which must always be present, why, in the latter, should we attribute this change to the agency of the vital principle, merely because it is present?

The action of a part is a change in the relative situation of its particles. How is this change effected, and how does this change, when effected, produce the various phenomena of living action? There are three circumstances which are essential to the existence of living action. A disposition in an organ to be excited into action, a stimulus to render that disposition effective, and a change in the relative situation of the particles which constitute its structure or organization. "Every operation of the vital energy is called an *action*, and whatever agent excites this action is called a *stimulus*. The aptitude of the system to be acted on has been expressed by the term, irritability."* I shall have occasion to advert to the opinion, here expressed, that every action is an operation of the vital energy. I quote the passage for the sole purpose of shewing, that the circumstances necessary to the action of an organ are its *irritability*, a *stimulus*, and the *action* itself, which I consider to be a change in the relative situation of the particles of matter which constitute the structure of an organ.

The irritability of an organ is its disposition to be acted upon, that is, to be influenced or affected by its appropriate stimulus. I have already said "The animal frame is composed of certain elementary substances; these, when united in certain proportions, constitute the solids and fluids of the body, and when moulded into certain forms, are denominated organs. These organs, in consequence of the peculiar arrangement of their elementary substances, are, under certain circumstances, endowed with the *power* of performing certain actions; and that this power or property of an organ is connected with its *organization*, we infer from the acknowledged fact, that the effects produced by different organs always correspond with the different arrangement of their elementary substances, in other words, with their peculiar organization." The *power*, then, of being excited into action, and the action itself, are considered to be dependent upon the organization of a part. We have already seen that life does not impart to an organ this susceptibility of action, because life exists in an animal, when the susceptibility of action does not; and because the susceptibility of action, which previously existed, may be lost, although the action of the vital principle, and the universally acknowledged effect produced by it, still continue. The organization, therefore, which is compatible with the union of life with matter, is not that organization which possesses the susceptibility of action. We find that seeds, snails, the horse-hair eel, the animalcule which resides in wheat, and the Vorticella Rotatoria, all possess life, but not susceptibility of action, in their *dried* states; when *moistened*, they acquire or recover the power of being excited into action. That this moisture unites with the organization of the seed, and of the animal, can scarcely be doubted. " If, however, moisture have access to them, they presently begin to *swell*, which is the first step towards their evolution."* " In a few hours, the seeds in both phials were much *enlarged,* and by the next day greatly *swollen.*" "The Rotifer shrivels up as the water evaporates, till it becomes like a piece of dried parchment; but on moistening it with water, it resumes its *pristine form.*"* It cannot be said that moisture acts as a stimulus, because if a seed possessing life, be kept moist at a certain depth below the surface of the earth, no living action is excited, and because moisture alone will not excite living action; another principle is necessary, viz. the oxygen of the atmosphere. It is almost unnecessary to prove what no one denies, that oxygen is essential to the existence of living action. Some authors, admitting this fact, have imagined that the agency of oxygen is wholly external to the body, and that it never enters into the system; others, that it is a stimulus, and excites the *action* of an organ; and others, that it only supports the irritability of an organ, that is, the power of being excited into action. The principal arguments advanced by Mr. Ellis, in favour of the opinion, that the oxygen is not taken into the system, are these:—"The quantity of oxygen lost by inspiration, corresponds with the quantity of oxygen contained in the carbonic acid expired; oxygen cannot be discovered in that blood which is imagined to contain it; oxygen cannot be supposed to have the power of penetrating the blood vessels; and the change produced in the colour of the blood admits of a different explanation. It would be wholly irrelevant to this part of my subject, to enter into a particular discussion of these several points. They are more immediately connected with the process of respiration, and the generation of animal heat; and I might fairly assume what so few physiologists have questioned, that the oxygen of the air does unite with the blood. But I may observe that the fact as to the quantity of oxygen lost, always corresponding with the quantity contained in the carbonic acid expired, is not universally admitted, and even if admitted, may be differently explained.* The oxygen contained in arterial blood may be incapable of detection by chemical tests, because it has entered into a new combination. If it be combined with electricity, there is no difficulty in conceiving how it may pass through blood vessels; and I do not think the change in the colour of the blood is satisfactorily accounted for, because, if venous blood lose its dark hue, in consequence of carbon being excreted from it, arterial blood ought to contain less carbon than venous, which is far

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* Burns.

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* Ellis's Inquiry.

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* Ellis's Inquiry.

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* Vide Respiration.

from being proved. The experiments of Professor Abilgaard led to a directly opposite conclusion. Few physiologists hesitate to admit, that oxygen unites with the blood, and that there is an intimate connection between the supply of oxygen and the action of an organ; but great difference of opinion is entertained as to the manner in which oxygen contributes to the support of living action. It has generally been supposed that oxygen acts a *stimulus* to the heart, but this idea involves many difficulties and contradictions. It involves the difficulty of imagining that the two sides of the heart are obedient to *different* stimuli, and yet, in amphibious animals, both sides of the heart are occasionally obedient to the same stimulus. In the fœtus, the right side of the heart is stimulated into action by the purest blood, and the left by that part which has circulated through the system, and is therefore more impure; but after birth the whole system is supposed to be reversed, and the right side of the heart is then supposed to be excited into action by the stimulus of blood which has circulated through the system and become impure; while the left side of the heart, which had previously been obedient to the stimulus of impure blood, is supposed to be then stimulated into action by pure blood only. "That the different sides of the heart require different stimuli, and that there is something peculiar in florid blood, which alone is capable of exciting the left side to action, we cannot, with Dr. Godwyn, admit. An objection presents itself that strongly militates against this opinion. Why should the same fibres, nourished by the same vessels, supplied with nerves from the same source, and performing the same function, be excited to action by different causes? This objection the Doctor is aware of, and attempts to remove it by observing, that the animal machine offers instances, where muscles of similar structure are put into action by different stimuli; but this is not saying that muscles, *performing the same functions*, act from dissimilar causes, which it is necessary to prove, before any analogy can be established in favour of this hypothesis. It is far, therefore, from being certain that the different sides of the heart derive their action from different stimuli; and if we examine the fœtal circulation, it will appear that both sides of the heart contract from the stimulus of blood nearly of the same quality."* Having described the peculiarities of the fœtal circulation, the same author says, "The right auricle propels part of the blood, which must be dark, into the left, and all the blood which passes through the capillaries in the lungs, also enters the left, so that the blood, which produces the contraction of the left side of the fœtal heart, must be more phlogisticated than that of the right, as part of the blood in the left auricle has passed through the lungs. If, therefore, the left side of the fœtal heart, and the whole of the arterial system, possess no stimulus but that of black blood; if the pulmonary artery in the adult be excited only by this blood; if, in a word, the hearts of fishes act with no other blood, is it not obvious, at least as far as inductions and analogies can prove, that in the adult also, venous blood can excite the action of the *left side of the heart* and arterial system, and consequently that the two sides of the heart do not require to be stimulated by dissimilar causes." Have we not proof that, in the adult, as well as in the fœtus, the *left side of the heart* is occasionally stimulated into action by *dark* blood? How could patients have recovered from apparent death, without artificial respiration, if the left side of the heart were absolutely incapable of being excited into action by dark blood? Mr. Burns says, " the use of the sides of the heart is, in one respect, the *re-*

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* Coleman on Suspended Respiration.

verse in the fœtus of what it is after birth. In the fœtus, the right side receives the purest blood, whilst the left receives it after birth. In the adult, the blood, which is in a state fit for circulation, is collected in the left side; and, therefore, the great artery of the body arises from that side."* The difficulties, the contradictions, and the improbabilities which this theory involves, are all obviated by the suggestion of M. Bichât, that oxygen does not act as a stimulus to the heart, but that both sides of the heart are obedient to the stimulus of blood quasi blood, that is considered without any reference to the oxygen which it may or may not contain. "It was long supposed, that the cessation of respiration occasioned that of the heart's motion, in consequence of the black blood not having sufficient power to stimulate its fibres; but does not the right side of the heart, which under all circumstances contains de-oxygenated blood, contract with a vigour equal to that of the left? It was reserved for Bichât to offer a true explanation of this phenomenon. He has very justly stated that, in consequence of the suspension of the respiratory functions—the coronary vessels—by which the muscular structure of the heart is supplied, are compelled to carry black instead of scarlet blood; a fact which, in itself, is quite adequate to explain the cause of the heart ceasing to contract; for the *irritability* of this, like that of every other muscle, can be alone maintained by duly *oxygenized* blood."* Professor Cuvier says, "Modern experiments have *demonstrated*, that one of the principal uses of respiration is to re-animate the muscular force, by restoring to the fibre its exhausted *irritability*."† Oxygen, then, does not *stimulate* the heart into action, but contributes to the support of its irritability, by which it is rendered *capable* of being excited into action by the blood, its sole and appropriate stimulus. In what manner does oxygen contribute to the irritability of the heart? Is it irritability itself? Does it unite with the heart, and then constitute its irritability? Or, is it only a necessary part of that organization which possesses irritability, as a property of its structure generally, and not of oxygen particularly? The irritability of the heart, is its property of being excited into action by the blood, its appropriate stimulus. We have no idea that oxygen can be thus excited into action, and therefore oxygen and irritability cannot be supposed to be the same things. We have no idea that oxygen, when united with the heart, acquires this property, and therefore we cannot suppose them to be the same things. Have we any proof that oxygen, or the arterial influence (to the excitement of which oxygen is necessary, and which, therefore, have been regarded as synonymous terms,) positively unite with the organic structure of the heart? As, in common medical language, we speak of the exhausted and accumulated irritability of an organ, we seem to consider the fact as not admitting of any question. The arterial influence contributes, in some way or other, to the support of that property by which an organ becomes sensible to the action of its appropriate stimulus. If, therefore, an organ, when removed from the system, and when cut off from all supply of this influence, still retain that peculiar property which we attribute to this influence particularly, it must be admitted, that the organ has carried the influence with it, or that our ideas upon the agency of this influence are altogether erroneous. The organ could not carry with it what did not appertain to it. But the arterial influence, except it unite with a peculiar organization, except it unite with *that* peculiar organization which we denominate vital or living, does not impart susceptibility of action. The organization, therefore, of a part, is as much concerned in that property, denominated irritability, as the arterial influence. I conceive, then, that I am justified in saying, "These organs, *in consequence of the peculiar arrangement of their elementary substances*, are, under certain circumstances, endowed with the *power* of performing certain actions." It seems, then, that life is consistent with an organization, which is too imperfect to possess susceptibility of action; but if to this peculiar organization, imperfect as far as living action is concerned, moisture and oxygen be added, that power, which we denominate irritability, is acquired. This, however, does not appear to be, strictly speaking, correct; for an organ may possess a living organization, moisture, and oxygen, and yet not possess susceptibility of action. A certain degree of temperature, varying in different animals, is essential to the existence of irritability. "Equally essential to this action is the presence and operation of *heat*. The ova of myriads of insects are evolved by its immediate influence, and its power is not less necessary to the maintenance, than to the beginning of living action. When abstracted to a certain degree, numerous tribes of animals pass into a torpid state, and again recover action as the heat is restored. Caterpillars, spiders, and ants, were many times in succession rendered torpid, and again restored to action in the experiments of Dr. Michellotti, by the alternate abstraction and communication of heat. In a temperature, 1° Reaumur, Spallanzani found living action in snails to cease; and the same result was obtained by exposing the marmot to a similar abstraction of heat. The heart moves quicker in hot than in cold animals, says Dr. Irvine; and in many animals, during the severe cold of winter, it *does not move at all.* The hearts of snails, which beat manifestly in summer, were found *to be perfectly at rest* in winter; and the same thing is observed in many of the fly tribe. Of the same description, too, are the serpent and viper tribe, frogs, toads, and tortoises; even the bat, which is naturally a hot animal, becomes, during the winter, as cold as the surrounding medium, and *its heart is perfectly at rest.* If an egg be opened, some days after incubation, so that the *punctum saliens* may come into view, according as it is exposed to heat or cold, says Dr. Mayhow, you will perceive the *corculum* or heart to pulsate, or to languish and cease from motion. The hearts of frogs, reduced to torpidity, were removed by Spallanzani, and living action could still be excited in them, by the re-application of a proper degree of heat; and the heart of a turtle, on being put into milk-warm water, was repeatedly observed by Dr. Gardiner, to yield a tremulous motion, six or seven hours after it was removed from the body, and had become much shrivelled and dried. If suffered to become cold, it was insensible to every stimulus; but when again warmed in water, it repeated its palpitations on being pricked with a needle. These, and many other facts which might be adduced, sufficiently establish the necessary concurrence of heat to the production of *action* in animals; and prove, likewise, that its abstraction, although causing a *suspension* of the animal *functions*, does *not* necessarily destroy the *capacity* of their *renewal*."* It is almost impossible to read this quotation, without observing the marked difference between life and living action, and without admitting, that the former is compatible with an organization which is too imperfect for the existence of the latter. If I were desirous of establishing the fact, that heat does not unite with the organic structure of animal matter, I should be accused of hazarding an opinion which is in direct opposition to all the acknowledged laws of the distribution of caloric, and, therefore, consistently with these laws, heat, under the circumstances mentioned, must be supposed to unite with the organization. It seems, then, that the imperfect organization, which we denominate vital, does not possess the *capability* of being excited into *action*, until moisture, oxygen, and heat, be added to it.

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* On Inflammation.

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* Med. Jurisprudence. † Comparative Anatomy.

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* Ellis's Inquiry.

Does the organization, in that more perfect state, which the union of these several substances imply, always possess the property in question—the capability of being excited into action? With regard to vegetables, the fact does not appear to be disputed; but immediately that we intrench upon the animal creation, physiologists look to another source of action—the nervous influence. It is of no consequence that, in the inferior animals, we can discover nothing like brain, spinal marrow, or nerves; that, according to the authority of Vicq, d'Azyr, Dumas, and others, "Polypes have no brain, no spinal marrow, no nerves," but "exquisite sensibility, if we may judge by the effect of slight impressions," that they have "no muscles, no muscular fibres, no articulated members," but "contractions frequent, rapid, reiterated; irritability quick, susceptible of being excited by the impression of light only." They have sensation, and therefore they *must* have nerves ; they have motion, and therefore they *must* have muscular fibres ; we cannot find either, and therefore we must *imagine* them. There is an undoubted connection, under certain circumstances, between the supply of the nervous influence and the phenomena of living action ; but this is very far from being a proof, that the nervous influence is, under *all* circumstances, essential to the existence of action. Many of the most eminent physiologists suppose the action of the heart to be wholly independent of the nervous influence ; and the facts in support of this opinion are so strong, that nothing but the difficulty of discovering an adequate function for the cardiac nerves, the insuperable difficulty of saying why, consistently with this opinion, the heart is supplied with nerves at all, seems to have prevented its general adoption. The heart is capable of being excited into action by its appropriate stimulus the blood, after the whole brain and spinal marrow have been destroyed. Dr. Wilson Philip says, "If the head and spine of a frog be removed, the heart continues to perform its function perfectly for many hours, nor does it seem at all immediately affected by their removal. But we find the effect very different when the most sudden and powerful agent is applied to them. If they are destroyed by being cut to pieces, or even by a hot wire, the heart, after their destruction, beats just as before it. But if either the brain or spinal marrow be instantly crushed, the heart immediately feels the shock."* Various experiments are recorded, which seem to justify the author in drawing the following conclusion. "In this experiment we see that the heart not only retains its power long after the brain and spinal marrow are removed; but if they are destroyed in such a way as to impair and almost destroy the action of the heart, it can *recover* the power of performing its function after they no longer exist; precisely as a muscle of voluntary motion will, by rest, recover its excitability, although all its nerves are divided, *if its circulation continues*."† It seems almost necessary to admit, that, under these circumstances, the circulation (which is supported by artificial respiration, and therefore, by a constant supply of that principle, which is necessary to the excitement, if not to the existence of the arterial influence) is the only source of action. It has been supposed, that the action of the heart, in these cases, may depend upon the nervous influence, which was accumulated in that organ, *before* the brain and spinal marrow were destroyed; but such an opinion appears to be inconsistent with the *renewal* of the heart's action by the supply of arterial blood, and with its *cessation* as often as the supply of arterial blood was interrupted; because, if the nervous influence, being the source of action, continued to reside in the organ, there does not seem to be any ostensible reason, why the action should have ceased at all. It has been supposed that the *nerves*, although separated from their communication with the brain and spinal marrow, may still continue to supply the nervous influence, and that the arterial influence supports the action indirectly, by being a necessary stimulus to the action of the nerves. But although the nerves, as I have endeavoured to shew, may have an independent action of their own, still there must be some reason why they communicate with the brain and spinal marrow. Manifest inconvenience appears to be *occasionally* derived from the circumstance, and, therefore, we must presume, that there is more than a corresponding advantage derived from it at other times. This advantage must be lost by the destruction of the brain and spinal marrow;

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* Philosop. Transact. 1815. † Ibidem.

and it is difficult to imagine that, with such a loss, the nerves should be capable of maintaining their action unimpaired. The acknowledged sympathy between all parts of the nervous system, renders such an idea extremely improbable; and the support of action by the arterial influence alone (I mean in those animals where no nervous system is apparent, and where we have a right to believe our senses in preference to our imagination) seems to be almost conclusive against the theory. What is the simple explanation of the fact? The arterial influence, when united with a certain organization, of which organization the nervous influence does *not* constitute a part, gives to that structure susceptibility of action, and therefore the nervous influence is not essential to the existence of this property. There are many other examples of living action in which, if we believe our senses, if we confine ourselves to what we do know, and exclude that which we do not know, the nervous influence seems to be not at all concerned. In a fit of apoplexy, for instance, all sensation and voluntary motion are lost, but the action of the heart still continues. Sensation and voluntary motion are the principal actions which we trace to the nervous influence.

These are wholly suspended, and therefore we may conclude that the supply of the nervous influence is suspended also; but the action of the heart continues. This action will be attributed by one physiologist to the arterial influence, which, from the continuance of respiration, we almost know to be present, and by another to the nervous influence, the existence of which is not only imaginary, but the *non-existence* of which may be inferred from the loss of sensation and voluntary motion. "M. Nysten a montré depuis que dans les paralysies *les plus completes* (does not this expression imply an absolute suspension in the supply of the nervous influence?) l'irritabilité se conserve dans les membres paralysés *tout aussi bien* que dans ceux que ne le sont pas. J'ai obtenu un résultat semblable d'une experience que j'ai souvent répétée."* Why, in order to explain this fact, should we *imagine* that the nerves still continue to supply their influence, when we have all the evidence which the nature of the case admits of, that their action is wholly suspended. The irritability of muscular fibre and the circulation continue, but the supply of the nervous influence, as far as we are borne out by what we are capable of observing, is positively suspended. If the circulation be also suspended, the irritability is soon lost. Can we require more evidence, that the irritability of muscular fibre is dependent upon the supply of the arterial influence, and that the nervous influence is not essential to the existence of this property? And we have this strong evidence in support of a fact, which, consistently with the custom of physiological reasoning, I might be justified in assuming, if by such an assumption I could more satisfactorily explain the phenomena of living action. "But if science were eventually to demonstrate that heat, light, and electricity, are different things, that there are various kinds of subtile substances; then I should be obliged to *suppose*, that there was also a subtile substance belonging to living bodies, a principle of life, which had the amazing power of kindling and controlling the destructive element of fire, and regulating the actions of that still more sudden and powerful agent, electricity. Neither, *by so doing*, should I transgress the rules of philosophy; for, in suggesting a theory, it should not only be probable, but adequate, to explain all the phenomena."*

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* Le Gallois.

M. Scarpa seems to consider the action of the heart in apoplexy to be so completely independent of the brain, (and this author, I may observe, has eminently distinguished himself by establishing the fact that the cardiac nerves penetrate and pervade the structure of the heart, and therefore cannot be supposed to have under-rated their influence,) that he considers it to be altogether similar to that which may be excited in that organ, after it has been absolutely separated from the body. "M. Scarpa, lui même, assimile les battemens qui ont lieu dans l'apoplexie, a ceux q'on observe, lorsque le cœur ne communique plus avec le cerveau, ni avec la moëlle epinière."* Am I not justified in drawing the conclusion, that the nervous influence is not essential to the existence of irritability? If this fact be admitted, there is no difficulty in perceiving why vegetables, which have no nervous system, may possess irritability; why the inferior animals, in which no nervous system is apparent, and the imaginary existence of which may fairly be questioned, possess this property, and why a paralytic limb may retain its irritability after the supply of the nervous influence has been absolutely interrupted.

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* Abernethy's Lectures.

It seems then that a peculiar organization may possess susceptibility of action without the nervous influence, and yet every organ of the body in man, and the superior animals, is most amply supplied with nerves. It would be wholly inconsistent with what I hope has been the general tenour of these observations, to suppose that the nerves are useless, or even to imagine that they have not a function to perform, corresponding with the universality, the extent, the ubiquity, of their distribution. M. Le Gallois says "Ainsi se trouvent expliquées les effets si singuliers en apparence de la destruction partielle de la moëlle epinière, et cette consequence, que j'avois deduite, que *deux conditions* suffisent pour entretenir la vie dans une partie quelconque d'un animal, savoir, l'*integrité de la moëlle epinière correspondante, et la continuation de la circulation*, demeure pleinement confirmée." I have endeavoured to shew that comparative anatomy, the phenomena of apoplexy and palsy, and the apparently decisive experiments of Dr. Wilson Philip, irresistibly lead to the conclusion that the influence of the nervous system is not essential to the existence of living action; that living action may and does take place without it; that the arterial influence uniting with a certain organization, imparts to it *susceptibility of action;* and I think it equally clear, that neither the arterial nor the nervous influence are essential to the existence of life, and that life may and does appertain to an organization, of which neither the arterial or nervous influence constitute a part. It would be wholly unworthy of my subject, the great and ultimate object of which is practical, to enter into any minor discussion of a comparatively immaterial point, that is, whether I am fully justified in saying, that life is consistent with an organization which does not contain *any* portion of the arterial influence, or something which corresponds with it. Oxygen, for instance, may be derived from our food. In torpid animals the arterial influence may continue to form a constituent part of the organization after respiration has ceased, after the supply of the arterial influence has been interrupted. I believe that life is consistent with an organization which contains no portion of the *nervous* influence, and I consider this fact to be established by the circumstance, that animals, in whom no nervous system is apparent, are evidently alive. If no brain, spinal marrow, or nerves can be demonstrated, I am surely justified in assuming the fact that they do not exist; it is certainly a much greater assumption to say that they do. I believe that life is compatible with an organization which contains *less* arterial influence than is essential to the existence of living action.

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* Abernethy's Lectures.

We have already seen, that if to a certain organization, which is compatible with life, but not with living action, moisture, oxygen and heat be added, that organization acquires the power of being excited into action ; but, generally speaking, the heat necessary to living action, must be superior to that of the surrounding atmosphere or medium in which the animal lives. This important subject, the production of animal heat, will be more fully considered hereafter ; I shall for the present merely observe, that if the arterial influence be a combination of oxygen with electricity, the source of animal heat is at once apparent. The secreting arteries impart *oxygen* to muscular fibre, by which the latter acquires susceptibility of action, and the electricity thus separated from the oxygen, being evolved in a free and uncombined state, is either caloric itself or at least a modification of that principle. The arterial influence, therefore, when applied by the living action of the system to its destined purpose, gives out both oxygen and heat, and therefore we might say, that if to a certain organization, compatible with life but not with living action, moisture and the arterial influence (that is oxygen and heat) be added, that organization acquires the capability of being excited into action by its appropriate stimulus. It will naturally be remarked, that the arterial influence cannot be considered as the source of living action, because it is only through the medium of living action, it is only by the *previous* existence of that action, that the arterial influence becomes the source of power. This objection is wholly inapplicable to the *commencement* of living action, because we manifestly discover in the parent the *previous* existence of that action by which the arterial influence becomes the source of power. The animal is not separated from the parent before it has acquired susceptibility of action, it is therefore not called upon to create but to support living action. We find that a certain time, varying in different animals, elapses between the formation of the embryo and its birth. The embryo acquires susceptibility of action from the parent at the moment of conception, but the organization is at that time too imperfect for the support of living action, and the fœtus is not separated from the mother until it has acquired a power of action equal to the difficulties with which, in its separated and independent state, it will have to contend. The great desideratum appears to be, a sufficient power to preserve the temperature of body necessary to living action, in opposition to that of the surrounding atmosphere, which has a tendency to reduce it to its own standard. The ovum, in birds, is separated from the mother before it has acquired this power, and if deserted in that state, will never acquire strength of action sufficient to preserve the temperature necessary to the support of life. The temperature of birds is very superior to that of the surrounding atmosphere, but the animal heat of fishes scarcely exceeds that of the medium in which they live. The ova, therefore, are separated from the mother and then deserted, because the power of action inherent in them is sufficient to preserve their temperature, in opposition to that of the surrounding medium in which they live. The difference between their animal heat and the temperature of the water is so trifling, that the ova can scarcely be said to have any opposition to contend with. "Exceptio probat regulam." The whale is a warm-blooded animal; its temperature greatly exceeds that of the surrounding medium, and therefore the embryo remains *in utero* nine or ten months before it is separated from the mother. Frogs, being cold-blooded animals, exclude their ova and desert them.

The mode of preserving eggs is a strong proof of the difference between life and living action. If absolutely excluded from all communication with the atmosphere, an egg may be kept alive for an almost indefinite period of time. We cannot suppose that living *action* is going on, because we know that action is a source of exhaustion, and can only be supported by a supply of oxygen equal to the expenditure. All communication with the atmosphere must be absolutely interrupted. We have the strongest possible proofs, that oxygen is necessary to the existence and the continuance of living action, but it is not necessary to the existence of life; and therefore an egg, excluded from the atmosphere, may *live* for an almost indefinite period of time. Torpid animals, in their inactive state, are in a condition precisely analogous with that of the egg. The latter is absolutely excluded from oxygen; the former have lost the power of converting that oxygen to the purposes of the animal economy, and, in some instances, have been excluded from all communication with it. A toad has been imbedded in a stone for years, and recovered the power of action when removed from it. It may be thought that a torpid animal in its inactive state, bears a nearer resemblance to an ovum which has been deserted by the parent bird, but which possesses, in itself, susceptibility of action, and is surrounded by an atmosphere from whence it derives oxygen, than to an ovum which is secluded from the atmosphere, and in which the susceptibility of action appears to be lost. As torpid animals, in many instances, appear to be wholly excluded from any communication with the atmosphere, the analogy suggested by the latter supposition, is clearly imperfect. The question appears to be, whether an animal, in a torpid state, *possesses susceptibility of action*, similar to the ovum which has been deserted by the parent bird, and to the ovum which is deposited by the fish and the frog, and then disregarded; or to the ovum which, for the purpose of preservation, has been excluded from the atmosphere, and is supposed to *possess no susceptibility* of action. It will be said that the ovum, in the latter instance, must possess susceptibility of action, because it may be hatched; but it cannot be hatched without the *addition* of heat, and therefore, without this addition, it does not possess susceptibility of action. The ovum deserted by the frog possesses, on the contrary, the necessary degree of heat which it had derived from the parent, or which it may derive from the surrounding atmosphere, the animal heat of the frog being often less than that of the air. The ovum deserted by the hen will putrify, because an action is going on which is unequal to the evolution of the chick, but which, at the same time, is destroying that organization, which is essential to the existence of life. There seems, therefore, to be a good reason why, in torpid animals, the evidence of our senses should be believed, and why no action at all is more consistent with the prolongation of life, than the imperfect action which has been imagined to exist. We have already seen that a torpid animal, when supplied with the necessary quantities of moisture, oxygen, and heat, will revive, but oxygen cannot *create* susceptibility of action ; some action must take place before oxygen can be rendered available for this purpose. We may suppose that at the *commencement* of the torpid state the supply of oxygen is greater than its expenditure ; it therefore accumulates in the system ; when all living action has ceased, no further expenditure of this principle can be imagined to take place, and therefore, upon the addition of heat, the susceptibility of action is present ; but still the animal in its torpid state, with this acknowledged accumulation of oxygen in the system, does not possess irritability, because a certain degree of heat is essential to the existence of that property, and that degree of heat is clearly not present. The great proportion of vegetables may be said to be in a torpid state during the winter. No action is observed, neither is it incumbent upon us to *imagine* that any action is going on, because life may exist without it. Upon what a simple law of nature does that beautiful *succession* of vegetation depend? Each plant has its peculiar temperature, at which it loses or acquires susceptibility of action. The great proportion of animals require a temperature superior to that of the surrounding atmosphere. The torpor of these animals, that is, the total cessation of living action, must, without artificial assistance, necessarily terminate in death. The Reports of the Society for the Resuscitation of Persons apparently dead, would lead us to suppose, that the total cessation of *action*, and absolute death, are not always synonymous terms. The heart may positively lose its susceptibility of action, in consequence of the necessary supply of oxygen having been interrupted, or from its temperature having been reduced below that standard which is essential to its irritability; and yet the organization of the heart may not have undergone that change which is incompatible with life If therefore oxygen and heat be added, the susceptibility of action *may* be recovered, although, for the time, it had absolutely ceased. This, however, is an extreme case. I am only suggesting, that if the arterial influence *could* be added to an organ, independently of a living action of the system itself, I have no difficulty in conceiving that the action of that organ might recommence, and that the total cessation of action does not imply absolute death. But it generally happens, that the action of an organ ceases, in consequence of its irritability being reduced below that standard which renders it sensible to the action of its appropriate stimulus. The action may have positively ceased, although the organ may still retain the power of being excited into action. It is unnecessary to say more upon this subject at present; but an important practical lesson may be derived from our always bearing in mind, that life and living action are not synonymous terms. We have already seen that every plant and every animal has a peculiar temperature which is essential to the existence of living action. There is no difficulty, then, in conceiving, how a *warm*-blooded animal *may* have a peculiar temperature at which it becomes torpid. The torpor of warm-blooded animals, in general, is nearly allied to death; but there may be a peculiarity in the bat, the dormouse, and the marmot, as to the temperature at which living action ceases; and it is probably owing to the passive state of the system, that this action ceases, before the organization compatible with life, has suffered any change, by an ineffectual endeavour to preserve its temperature.

If to a certain organization, (*not* possessing the nervous influence,) moisture, and the arterial influence be added, susceptibility of action may be acquired. Why, then, do nerves constitute so essential a part of the animal structure in general? I have already stated that nerves are not indispensibly necessary to the existence of living action, and have given my reasons for dissenting from the argument so commonly adopted by physiologists—that, where sensation is, nervous matter must exist. "In some of the most simple animals of the class Vermes, particularly among what are called Zoophytes, little or no distinction of similar parts or structures can be discerned, and we are unable to recognize any thing as a particular nervous system, or even as a part of such a system. The power of sensation and voluntary motion, which these possess, as well as any other order or class of the animal kingdom, *proves* that the nervous matter *must* be uniformly spread throughout their homogeneous substances."* "To me, however, who confide more in the eye of reason than in that of sense, and would rather form opinions from analogy than from the imperfect evidence of sight, it seems too hasty an inference to conclude, that in the minute animals, there are no vessels nor other organization, because we cannot see them; or that polypes are actually devoid of vessels, and merely of the structure described, because we can discern no other. Were it, however, really so, such facts would then only shew, with how little, and with what various organization, life could accomplish its principal functions of assimilation, formation, and multiplication."† Is it not more consistent with *reason*, to assume the fact, that nerves do not exist where we cannot demonstrate them, than to assume the fact that they *must* exist, because we cannot explain the phenomena of sensation without the assumption? I shall, for the present, assume the fact, that nerves are not *exclusively* the organs of sensation. The subject will be more fully considered in the chapter appropriated to the sensitive functions.

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* Encyclopedia. † Physiological Lectures.

Having already given my reasons for supposing that the arterial and nervous influences are the same things, I shall here confine myself to the arguments founded upon the phenomena of living action. One physiologist attributes the action of the heart to the arterial influence exclusively, another to the nervous influence, and another to both conjointly. The idea very naturally suggests itself, that they may be the same things. I have endeavoured to prove, that we cannot suppose them to be different, without attributing to the brain and nervous system, an action which is inconsistent with all analogy, and with the anatomical structure of these organs. If I find that, under certain circumstances, the action of the heart is evidently supported by the *arterial* influence alone, and that I cannot deny the fact without having recourse to my imagination; without believing that nerves may exist, although they are not apparent; that the brain may continue to act, although I have all the evidence which the nature of the case admits of, that it does not; that the nerves communicate with the brain for no good purpose; and that this communication may be destroyed without impairing their action; if I find that, under certain circumstances, the action of the heart is evidently supported by the *nervous* influence alone, and that I cannot deny the fact, without transgressing the established physiological axiom, that the *same* action, in the *same* organ, is always excited by the *same* stimulus; if, in order to account for the fact otherwise, I am under the necessity of supposing that *blood* and the nervous influence (substances as distinct as any two in nature) produce the *same* effect in the *same* organ; if, in order to explain the fact otherwise, I am under the necessity of supposing that the nerves impart an occasional *stimulus* to the heart, that is, a source of further exhaustion, at a time when the heart evidently wants support; if all these difficulties are avoided, by simply believing that the effect produced by the nervous and arterial influences being the same, the cause is also the same, and that they are in reality the same things; if, under certain circumstances, the action of the heart cannot be carried on without this secondary source of power; if, under ordinary circumstances, the action of the heart cannot be carried on by a single source of power, without constant and imminent danger of disease, and if this danger be avoided by the *same* power being derived from another source; if, in short, we have all the evidence which the nature of the case admits of, that the fact is so, that evident advantage is derived from its being so, and that it is necessary that it should be so, upon what grounds shall we deny that it is so?

Living action is supported by the arterial influence alone in animals which have no brain or nervous system, but whose respiration is proved by the changes they induce on the atmosphere; that is, by the loss of oxygen and the substitution of carbonic acid. It is of no consequence to my argument that I cannot prove the existence of blood vessels; there may be none; nature may have some other means of distributing the influence in question; but I have *proof*, as far as experiment and analogy go, that *all* animals respire, and therefore *all* animals produce that change in the atmosphere, which we admit to be the source of the arterial influence. Considering the arterial and nervous influences to be *distinct* things, from what apparent source, from what evident *action* of the system is the latter derived? It may be said that analogy justifies us in supposing, that all animals have nerves—granted—*all* animals *may* have a secondary source of power; but the evidence of analogy *alone* is very similar to that of an accomplice in guilt, which evidence no one thinks of believing unless it is strongly corroborated by that of others. It may be said that the arterial influence is admitted to be present, and that *this* is an evident *source* of the nervous influence. If I have no other reason for supposing that the arterial influence *is* generated, than that the atmosphere is present from which it *may* be generated, will such an argument be admitted? The arterial influence, from whence that of the nerves *may* be generated, being present, is but a weak argument in favour of the opinion that it *is* generated. We admit that all animals respire, and consequently that all animals have respiratory organs, *because* we can prove that the air undergoes those changes which are produced by respiratory organs alone. But have we any proof that blood in animals, without an apparent nervous system, undergoes those changes which are produced by the nervous system alone. This is so far from being the case, that even where nerves are apparent, the arterial influence is not imagined to be *wholly* expended upon them. We have therefore a powerful argument in support of the opinion that the *arterial* influence is generated, which argument does not apply to the *nervous* influence. But sensation is present, and sensation, it is said, always implies the action of nerves. As I do not admit that sensation is an action of nerves *exclusively,* this argument is to me no argument at all. As it does not appear to me that the nature of living action in zoophytes requires a secondary source of power, but that such a secondary source is indispensible in the superior animals, I have not the argument of usefulness to add to that of analogy, in support of the opinion that nerves exist where they are not apparent; and I conclude that living action is supported by the arterial influence alone, because the arterial influence appears to be adequate to the effect produced.

Living action is supported by the arterial influence alone in apoplexy, probably in the fœtus, and apparently in the egg. It has been supposed that sensation always implies the action of nerves; if there be any weight in this argument, the absence of sensation must imply the absence of nervous action. Voluntary motion is another indication of nervous action. The loss of this power must, therefore, imply the loss of that action. Sensation and voluntary motion are lost in apoplexy, but the contraction of the heart still continues; one physiologist, however, attributes this action to the arterial, and another to the nervous influence; the contraction of the heart, therefore, is no proof that it continues to receive the nervous influence. As far as we can judge from those phenomena which have been attributed to the nervous influence exclusively, we must conclude that all nervous action is suspended; and this opinion I have shewn to be not unsupported by the authority of other writers.

Living action appears to be supported *in the fœtus* by the arterial influence alone, which is derived, through the medium of the placenta, from the mother; but no author imagines that the nervous influence is communicated in the same manner. One writer says, "the fœtal existence is *purely vegetative*. To say that it is asleep is erroneous; for not only are the *organs* of sense and voluntary motion in a state of *perfect repose*, but also several of the assimilating functions are totally unemployed, as digestion, respiration, and the generality of the secretions."* Dr. Monro says, "While fœtuses continue in the womb, their muscles are ordinarily left to act only by their natural contraction, or the fœtus is said to be in a *sleeping* state. But sometimes, when its ease or preservation require a change of situation, it *seems* to perform some voluntary motions, which are called its stirrings."† Mr. Burns says, "Where the life is of the highest kind, but its quantity naturally small, and the *action required*, at a particular time, *is very little*, we likewise find the blood impure. This we see to be the case of the fœtus in utero, where the *action required is very trifling*," &c. The absence of sensation and voluntary motion, and the nature of the action required, would seem to imply that the nerves do not act, and that their action is unnecessary; and the state of the blood being impure would lead us to suppose, that the small quantity of arterial influence which it does contain, is not unnecessarily expended upon the organs of sensation and voluntary motion, which are "in a state of perfect repose."*

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* Encyclopedia. † Monro's Works.

Living action is supported by the arterial influence alone in eggs. I am not aware that any author has even *imagined* the action of nerves in the first stage of incubation; but the respiratory process is proved to be going on. "These facts *prove* that a reciprocal action is going on at all times, between the carbon of the egg and the oxygen gas of the air, by which carbonic acid is produced."†

Living action is supported in vegetables without nerves, and there is scarcely a modern physiological work, in which "the purely vegetative life of the fœtus" is not alluded to. If, then, the phenomena of living action are apparent, where there is no brain naturally, where the brain has been artificially destroyed, where the action of the brain is suspended by disease, where the nerves are not imagined to act, where the nerves are scarcely imagined to exist, and in plants where their existence is absolutely denied, how can we possibly attribute that living action to the influence of a fluid secreted by the brain? But if under all these circumstances, we have positive proof of the existence of that living action by which the air is decomposed, and which is acknowledged to be the first process in the generation of the arterial influence; and if the agency of the arterial influence (such as it is acknowledged to be by some of our best writers) is sufficient to account for the phenomena in question, why should we attribute these phenomena to any other cause?

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* I do not deny that nerves may correspond with other parts of the body in being organs of sensation. I have only objected to the term being applied to nerves exclusively.

† Ellis's Inquiry.

Living action, immediately after birth, until the period of dissolution, is supported by the arterial and nervous influences *conjointly*.

Animals which have no nervous system are, of course, exceptions to this rule, and I have already endeavoured to shew, that, in some diseases of the brain, living action is supported by the arterial influence alone. But in infancy and childhood, living action is *more* dependent upon the nervous influence, than at any subsequent period of life. Every anatomist, who wishes to demonstrate the nerves, selects for that purpose a *young* subject, and, by so doing, practically admits that, comparatively speaking, the nerves are more fully developed in the infant and child, than in the adult. These organs, being more fully developed, may be presumed to have a more important function to perform. It may be argued (for what may not ?) that the nerves, although *apparently* more fully developed, are in reality imperfect in their structure, and that we cannot, from this apparently more perfect development, infer the existence of their more important action. But I am one of those who imagine, that no peculiarity of structure can be invariably the same without design; and when I see design in the works of nature, I always infer, that the advantages derived from that design are greater than the inconveniences. In consequence of the more sensitive state of the infant system, a state which is by these authors attributed to the nerves, a spasm in the bowels, which is scarcely regarded by the adult, will often lead to a chain of actions by which life is destroyed. As life is occasionally destroyed through the agency of these nerves, we must presume that at other times it is preserved by them ; and as the occasional inconveniences are great, we must presume that the more permanent advantages are great also ; and if so, the action of the nervous system is more important in the infant than in the adult. The nerves are supposed to be the exclusive organs of sensation and voluntary motion. Now, in the infant, the organs of sense are, comparatively speaking, more useless than at any subsequent period of life ; and voluntary motion does not appear to exist, or if it exist, is clearly of no vital importance to the infant. The eye does not perceive, or if it perceive, derives no apparent advantage from it; the touch perceives but cannot discern; the ear derives no advantage from hearing ; the taste is comparatively unimportant, and the smell appears to be absolutely useless ; and yet the organs subservient to these different senses are particularly subject to disease, in consequence of the increased sensibility which they possess. It appears, then, that the more perfect development of the nervous system in infants, is not attended with a corresponding perfection in the organs of sensation and voluntary motion. The term "more perfect development of the nervous system" will probably be objected to. I shall be told that the brain is in a dissolved, and in an almost fluid state; but fortunately this is an argument of no weight; because we all admit the fact, that the infantile system *is* more sensitive than that of the adult. It would involve a strange kind of argument, to admit that a thing is, and then to say that it is not, because we do not see why it is. We admit that the infantile nerves are more sensitive than those of the adult. This fact is not disproved by our saying, that the brain is so organized, that we cannot conceive why the fact is so. To say that the infantile system is more *irritable*, considering irritability as a property wholly independent of the nervous system, would be assuming a fact which we cannot prove, which is unsupported by all analogy, and which is inconsistent with the comparatively *imperfect* state of the circulating, and the comparatively perfect state of the nervous system. If, then, the more perfect development of the nervous system in infants, which implies more perfect action, does not lead to a corresponding perfection of those functions, (sensation and voluntary motion,) which are attributed to the nervous system exclusively, it is difficult to imagine that the nerves can be the sole organs which influence sensation and voluntary motion, because the action being more perfect, the functions dependent upon it, ought, in this respect, to correspond with it. It will no doubt be said, and probably with truth, as far as sensation is concerned, that the imperfection lies in the mind; but this explanation, if applied to the phenomenon in question, leads to the inconsistency of supposing, that the nervous system has been more fully developed, has been rendered more active and energetic at a time when, from the imperfection of the mind, this energy and activity are absolutely useless. If we suppose the nerves to be the organs of sensation and voluntary motion only, I do not see how we can reconcile the supposition with the fact, that the nerves are more fully developed, that their action is more energetic, at a time when sensation and voluntary motion are evidently imperfect. To explain the fact, *why* the nervous system is more energetic in the infant than in the adult, we must look not to the mind but to the body, not to the action which is *imagined* to be communicated from the extremity of the nerve to the brain, but to that which is communicated by the nerve itself to the system at large.

We have proof that the action of every organ is, to a certain extent, and with some exceptions, dependent upon the nervous influence. What advantage, for some advantage there must be, can the infant derive from having a system more sensitive than that of an adult, from its actions being more dependent upon the nervous influence, than they are at any subsequent period of life? The action of the heart is supposed to be dependent either upon the arterial influence alone, upon the nervous influence alone, *to the excitement of which the action of the heart is necessary*, or upon both conjointly. The action of the heart, then, is solely dependent upon itself; because the arterial influence can only be communicated to the heart by its own action, and that action is necessary to the excitement of the nervous influence. If, therefore, the power of the heart be exhausted by over action, how is that power *regained?* The heart must still work on, must still further *exhaust* itself in order to restore its power, because, without this *increased* action of the heart (that is, without an action greater than natural) the arterial or nervous influence cannot be supplied in that increased quantity, without which the organ can never be restored to its original power. So that the organ is called upon to *exhaust* itself in order to restore its power. The inconsistency of such an action for such a purpose, and the want of harmony between the object to be accomplished and the means by which it is brought about, are, in my opinion, the strongest possible proofs that such is not the mode by which the heart recovers its power. It is a libel upon nature to imagine it!

If, in this state of exhaustion, a stimulus be applied to the nervous system, through the medium of the stomach, or any other organ, the action of the heart becomes firmer, steadier, and more energetic; the heart has evidently recovered power. But the nervous influence is imagined to act as a *stimulus* to the heart: how can a *stimulus* increase power? How can a cause, which produces action as an effect, restore that power which was exhausted by action as a cause? How can action exhaust and restore power at the same time? Is it not clear that, under these circumstances, the nerves must have given power, and not action? By what process, then, does the heart, in a state of exhaustion, recover power without any increased and debilitating action of its own? The nervous influence is a secondary source of power; nature has provided for this difficulty, by accumulating, at a time, when the supply of power is greater than the exhaustion (in sleep for instance,) the *arterial* influence, which we have already seen to contribute, not to the *action* of the heart, but to its irritability, its *susceptibility* of action. This *arterial* influence being accumulated in organs called the brain, spinal marrow, and nerves, has been denominated the *nervous* influence; but they are in reality the same things. I have already given, at some length, my reasons for entertaining this opinion; but there is one, arising from the phenomena of living action, which appears almost to establish their identity, and at the same time throws considerable light upon the question proposed. "By what process does the heart, in a state of exhaustion, recover power without any increased and debilitating action of its own?" It will readily be admitted, that syncope and asphyxia are, as far as the action of the heart is concerned, modifications of the same disease. "Syncope cognoscitur subitaneâ virium omnium vitalium, animaliumque imminutione; pulsu scilicet, respiratione, omni musculorum motu, necnon sensu quovis, et calore admodum imminutis, ita, ut æger per aliquod tempus sui conscius non sit, et vita eclipsin patiatur. In asphyxiâ ægri tamdiu frigidi, sine ullo signo vitæ remanent, ut pro mortuis sœpe habeantur; ast syncoptici sudore, pulsu obscuro, aliquali interiorum calore, leni saltem respiratione, ab asphyctis distinguuntur, et intra minutum unum aut alterum ad se redeunt."* Dr. Paris says, "In ordinary fainting, it is evident that some slight and feeble motions of the heart still continue, although insufficient to produce a sensible pulsation in the more distant arteries. But the motions of the heart *may have ceased altogether;* and in such cases it becomes a question no less interesting to the practical physician than to the physiologist, whether they can ever be restored; and, if so, we have to inquire, under what limitation as to time; under what circumstances; and by what means." It is wholly unnecessary to enter into any discussion of the point, whether the pulsations of the heart, in these two diseases, are so weak as not to be perceived, (although they continue,) or are absolutely suspended; or whether they continue in the one, and are suspended in the other. The only fact to be established is this; is the heart too weak to propel its contents? "Where syncope arises from other causes, the heart is seen distended to an unusual magnitude, and the blood in the left auricle and ventricle is generally of a more or less florid colour, and has not the hue of venous blood; a circumstance which depends upon the pulsation of the heart ceasing before the function of respiration, and which is the very reverse of what happens in death from suffocation."* "In death from suffocation, the heart continues to pulsate for several minutes after the breathing has entirely ceased, in consequence of which the blood, which passes through the pulmonary vessels, no longer receives the influence of oxygen, and therefore black blood circulates."† It is of no consequence to my argument *when* the action of the heart ceases. The action ceases either positively or apparently, before respiration in the one, and after respiration in the other. Why does the action of the heart positively or apparently cease? Because, that organ is too weak to propel forward its contents. We see that in syncope "the heart is distended to an unusual magnitude, and the blood in the left auricle and ventricle is generally more or less of a florid colour." The action of the heart then, does not cease for want of stimulus; the blood, its appropriate stimulus, being accumulated in that organ. Dr. Paris, speaking of asphyxia, says, "It is not until after the full effects of the suspended respiration are thus produced on the brain, that the motions of the heart become *enfeebled*, and that the ventricles contract *less powerfully*, and at longer intervals. At length the action of the heart is *altogether arrested;* and if the thorax be examined at the instant that the circulation has ceased, nothing is observed except a slight tremulous motion of the auricle; the cavities of the left side are much contracted, and contain only a small quantity of blood, while the right auricle and ventricle, and the large vessels communicating with them, are distended to an unusual size."* The motions of the heart, then, are not "enfeebled or arrested" by the want of their appropriate stimulus, the blood. The heart, in both these diseases, may be considered to be in precisely the same state, the difference in the colour of the blood not at all affecting its stimulant property. The power of the heart is too weak to impel forward its contents; how is that power so far increased as to restore the circulation? By artificial respiration in one case, and by a stimulus applied *to the nervous system* in the other. Is then the *same* effect, in the *same* organ, produced by two different causes? Is it not more rational to suppose, that the effect being the same, the cause is also the same? Such indeed appears to be the simple explanation of the fact: the *arterial influence* is supplied through the medium of the respiratory organs in one case, and through that of the nervous system in the other; by artificial assistance in the one case, and by increased action of the nervous system in the other; and the *arterial* influence derived from either of these sources unites with the heart, and increases, consistently with what has been already said, its *susceptibility* of action, that is, its power. The stimulus of the blood, which was incapable of exciting the action of the heart in its exhausted state, is sufficient to excite that action, when the disposition of the heart to be acted upon is increased. If, then, the nervous influence be a secondary source of power, be in reality the arterial influence, accumulated in the nervous system during a state of comparative inaction, ready to be employed according to the exigencies of the system, that is, when a comparatively increased action of the system leads to its exhaustion, we at once see why the infantile system, being, from its delicate organization, *sooner* exhausted, should, in a peculiar manner, require this secondary source of power; why the inferior animals, whose actions are almost vegetative, and absolutely uninfluenced or discomposed by feeling, (for the loves of an oyster and of a plant appear to be equally fictitious and imaginary) should not require it at all ; and why the nervous system should be more fully developed in the infant, and be altogether wanting in the zoophyte. It would be anticipating my subject to point out the extensive application of this doctrine to pathology. It clearly involves some of the most leading and important points, and some of the most embarassing difficulties which present themselves in practice. What, for example, constitutes the difference between fever and irritation, between that diseased state of the system which can only be counteracted by extensive depletion, and that which can only be *quieted* by a *stimulant ?* How does it happen that the symptoms of all kinds of fevers, from whatever causes they may arise, are apparently so similar, and yet require a directly opposite mode of practice? Why does a certain degree of pressure upon the brain produce a slow, labouring, and irregular action of the heart, and a greater degree of pressure lead to a directly opposite state of action? How shall we reconcile the fact, that the reaction of the system in disease is often the ostensible cause of death, with another fact, (so obvious to the eye of reason that, in my opinion, it does not admit of being questioned,) that, the cause of disease being applied to the system, the *first* link in the chain of diseased action, is *always* connected with a salutary tendency. But although the discussion of these several points would, at present, be premature, it is not irrelevant to this part of my subject, to advert to the apparent necessity of a secondary source of power; to point out in what manner this accumulation of the arterial influence *prevents* disease. I have already alluded to the glaring inconsistency of that doctrine, which supposes that the heart can restore its own power by the selfsame means which exhausted it; that action (a manifest source of exhaustion, and admitted to be so, not upon any theoretical principles, but upon the irresistible conviction of our feelings,) should be capable of exhausting and restoring power at the same time. Admitting, for the sake of argument, that the heart, by a *debilitating* action, is *capable* of supplying *more* power than it expends; nature must be a miserable architect indeed! She is employed to build a house, engages one labourer to put up three bricks, and pays another for taking down two. Let us suppose that these difficulties are imaginary; that the heart is the *only* source of power to the rest of the system, and that nature has been so improvident as not to have anticipated the difficulty of being called upon to provide power by increased exertion, at a time when that increased exertion must be an *additional* source of exhaustion. It is placing nature in the situation of an individual, whose support is wholly dependent upon his own exertions, who has provided no resources, and who, being unfortunately seized with fever, is desired to get up and work, or starve. It has been supposed that the heart is a muscle which never fatigues. "Cor ipsum, validissimus musculus, centies millies quotidie micat, post octaginta annos, nihilo magis fatigatum."* This observation, however, applies only to the natural and the healthy action of that organ, for the author immediately adds, "Post vehementem vero, aut *insolitum* aut *abnormem* motum, inviti etiam motûs musculi quodammodo *debiliores* evadunt." After the usual exertions of the day, we feel a sensation of fatigue, a disposition to sleep, and an almost insuperable disinclination to further exertion. We fall into a state of insensibility and comparative inaction, and after a certain time awake; but the sensation of fatigue, the disposition to sleep, and the disinclination for

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* Sauvage's Nosology.

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* Med. Jurisprudence. † Ibidem.

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* Med. Jurisprudence.

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* Conspectus Medicinæ Theoreticæ.

exertion are no longer remembered. No argument is necessary to prove, that the supply of power has been greater than the expenditure; power has been accumulated. Let us suppose that, from some cause, sleep is prevented, and that in this exhausted state of the system, action still continues; what is the consequence?—Fever. "Pulsus frequens, calor major, plures functiones læsæ, viribus præsentim artuum imminutis." Is it not clear that these symptoms arise from the system being called upon to supply power, when, in its exhausted state, it is comparatively unequal to the exertion? The symptoms will be nearly the same, whether the powers of the system be exhausted or depressed. But if the powers of the system have been neither exhausted nor depressed, increased action of the heart and arteries, and excess of heat, do not constitute fever. Now if there be no accumulation of power in the system to answer the demands of unusual exertion, we could not run a hundred yards without the commencement of fever; but if the essence of fever consist in the exhausted or depressed state of the system, we see why this unusual exertion having been provided for by a certain accumulation of power, increased action of the heart and arteries, increased heat, and *a certain degree* of exhaustion do not constitute fever. We cannot deny that power is accumulated in sleep; that it is raised from exhaustion, the consequence of action, to its natural standard. Why should nature, possessing the means of accumulating power, have been so niggardly in her supply, as not to have provided for the expenditure of the day, without the inconsistency of calling upon organs to restore their power by an action, which had previously exhausted it? We are told that every increased action of the heart and arteries, however slight, constitutes the phlogistic diathesis, that is, an inflammatory disposition. "Frequentior, et majoris momenti vigoris excessus, observatur in fibris musculorum qui voluntatis imperium non agnoscunt, præsertim in iis qui sanguinem movent. Hujus circuitus sœpè præter solitum intenditur, non sine gravi valetudinis et vitæ incommodo ant periculo. *Levior hujusmodi excessus*, totum corpus pervadens, homines morbis inflammationem habentibus opportunos reddit, et diathesis phlogistica vocari solet."* If the power of a muscle be solely dependent upon its artery, every action of a muscle, being attended with exhaustion, must be attended with an increased action of its artery to restore the power which has been exhausted; and thus every action of a muscle must be attended with an *inflammatory* disposition of its artery. How is this inconvenience guarded against? There is a secondary source of power in the nerves. They communicate the *arterial* influence to the exhausted fibre, and thus a generally increased action of the heart and arteries does not become necessary, until the accumulation of power in the nervous system, is reduced below the standard of health; and then, but not upon every slight motion of the body, the inflammatory diathesis commences; so that the increased action of the heart and arteries, both in fever and inflammation, is dependent upon the same cause, viz. a depressed or exhausted state of this secondary source of power, denominated the nervous influence. But we find, that the inconveniences, which such a state of the system would lead us to anticipate, do actually occur, when that state of the system is, to a certain extent, its prevailing character. Thus, in infancy, the actions of the system are disproportioned to its power, and exhaustion is constantly taking place. If this exhaustion could only be restored by an inflammatory action of the arteries, (and we are told that every increased action of the arteries constitutes an inflammatory disposition) the infant would be necessarily and uniformly in a state of disease. And yet we seldom or ever hear of ossification of the arteries in infants, which would be almost a necessary consequence of this state of action. In old age the system is reversed, as far as this secondary source of power is concerned, but is in a state precisely analogous with that of the infant, if we look to the general power of the system. Action is, in both, quickly followed by exhaustion; but in one, the infant, the nervous influence (the secondary source of power) is at its maximum, in the other, at its minimum. It is unnecessary to adduce any proof in support of the fact, that the energy of the nervous system is diminished in old age. What is the consequence of this state of the system? there is but one source of power, (comparatively though not absolutely speaking,) and that is supplied, not by the nerves, but the arteries. Every increased action of the system therefore necessarily involves an increased action of the arteries, and ossification of these vessels, is, more or less, the universal consequence of this state of the system. Upon the same principle I should explain the fact, that women are, in a peculiar manner, almost exempt from angina pectoris. I am of opinion that, without this secondary source of power, the animal frame would never be free from fevers, inflammations, and the whole train of complaints which are dependent upon diseased action, or diseased organization of the circulating system.

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* Conspectus Med. Theoret.

If, then, to a certain organization which we denominate vital (and which may be so denominated without implying susceptibility of action, or, at least, the power of being excited into action by its *appropriate* stimulus) moisture, heat, and the arterial influence (communicated either by the circulating or nervous system) be added, that organization is always endowed with the capability of being excited into action. It might be a curious and not uninteresting inquiry, to investigate this subject still farther, with the view of ascertaining at what precise degree of heat, moisture, and oxygenation, the different classes of the vegetable and animal creation acquire and lose this property. The succession of vegetation, and the torpidity of animals under circumstances apparently different, could not, I think, fail of being elucidated by the inquiry. The experiments of M. Le Gallois led to some results which were as unexpected as they are singular and important. I allude more particularly to the fact which he has established, that *very* young animals retain life after the circulation has been destroyed, *longer* than those of a more advanced growth. "La mort ne survient jamais, soit dans une partie, soit dans tout le corps, aussitot que la circulation y à été interceptée, mais seulement au bout d'un certain temps. Ce temps, qui est determiné dans les animaux de même espèce et de même âge, est d'autant *plus long* dans ceux à sang chaud, qu'ils sont *plus voisins* de leur naissance. Aussi, lorsq'on arrête tout à coup la circulation dans les lapins soit en liant, soit en arrachant le cœur, la sensibilité ne s'etient qu'au bout d'environ quatorze minutes, quand ils sont nouvellement nès; au bout de deux minutes et demie, quand ils ont quinze jours; et au bout d'une minute, quand ils en ont trente."* Does not this fact strongly indicate a secondary source of power, and that this power is efficient in proportion to its necessity, that is, to the tender age of the animal. The same author says, "*il est certain* que la vie n'est pas dans le sang, comme on l'a dit souvent, et que la circulation ne la constitue pas essentiellement, puisque le sentiment et le mouvement volontaires subsistent toujours, un temps quelconque, après l'arrachement du cœur, et en general après la cessation de la circulation."* It may be said, that the muscles *retain* their irritability, after *all* supply has been intercepted, longer in the animal which is newly born, than in those of a more advanced age; but such an explanation is inconsistent with the fact, that the muscles of a young animal lose their irritability *sooner* than those of its parent: they are sooner exhausted. It is also in opposition to all analogy; for we find that the irritability of muscular fibre is most durable and permanent in those animals, whose nervous system is the most imperfectly developed, and the least energetic. Nature seems, in a peculiar manner, to have protected the life of the infant; for not only does it enjoy this secondary source of power in greater perfection, but it still retains some of the peculiarities which characterized its fœtal state. We have already given our reasons for supposing, that the "purely vegetative existence" of the fœtus, may be supported by the arterial influence alone. Consistently with this idea, we find, that if the spinal marrow of an animal, *one day old*, be pierced with an instrument, *that* animal will retain its sensibility for *sixteen minutes*; but if the same experiment be repeated in an animal *six days old*, *that* animal

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* Experiences, &c.

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* Experiences, &c.

is insensible *in six minutes*." "Moëlle epinière coupée avec une aiguille entre l'os occipital, et la première vertèbre, aussitôt tous les mouvemens inspiratoires sont anéantis et remplacés par des baillemens. L'animal s'agite pendant un peu plus d'une minute; apres quoi il demeure sensible de tout le corps. La sensibilité s'etient vers *seize* minutes." "Moëlle epinière coupée avec une aiguille entre l'os occipital et la première vertèbre. La sensibilité cesse a *six minutes*, et les baillemens a sept."* It appears, then, that the two sources of power are eminently conspicuous, and, in a remarkable manner, combined in the infant.

If, to an organization possessing *susceptibility* of action, a stimulus be applied, action itself is the consequence. We say that blood is the appropriate stimulus of the heart, aliment of the stomach, fœcal matter of the intestines, and urine of the bladder. But if we look to the *effects* produced by these different substances, we shall have reason to suppose that the *immediate* stimulus of every organ is the same. The mechanical effect of each is to *distend* the organ to which it is applied. Distension implies a change in the relative situation of the particles which constitute the structure of an organ, and this change I should denominate its stimulus. Instead, therefore, of saying that blood is the stimulus of the heart, I should confine that term to the distension of its muscular fibres; and it seems necessary to make this distinction, because the heart may be excited into action without blood; for instance, by a variety of mechanical or chemical stimuli. How, then, is voluntary motion produced? Each voluntary muscle is naturally in a state of *distension*, which may be proved by that muscle contracting upon itself, when wounded or separated from its attachment. Each muscle, therefore, contains in itself its stimulus to action; and if, by the will of the animal, the susceptibility of action be increased, *that* stimulus will produce the contraction of muscular fibre. So that the effect produced by the attachment of a voluntary muscle, and by the distending impulse of the blood, is precisely the same. I imagine that every stimulus produces a change in the relative situation of the organic particles to which it is applied. It is unnecessary to enter more fully upon this subject at present, but an important question presents itself. Is the nervous influence to be regarded as the stimulus of muscular fibre, or does it correspond with the arterial influence, in simply adding to its irritability?

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* Experiences, &c.

I stated that the heart was capable of being excited into action by *different* stimuli; but I then alluded to the contraction which may be excited by various means, when that organ is separated from the body. In its *natural* situation, each organ has its sole and appropriate stimulus; that is, the change in the relative situation of its particles (the *immediate* stimulus) is produced by one and the same agent in the same organ; although in different organs the stimulus, that is, the cause of distension, is different; so that if the blood be regarded as the stimulus of the heart, the *attachment* of voluntary muscles may be regarded as their stimulus—they are the different agents by which, both in the heart and in the voluntary muscles, a change in the relative situation of the particles is produced. The same effect is produced by different agents in different organs, but always by the same agent in the same organ. Dr. Wilson Philip, (whose ingenuity in devising, assiduity in performing, and acumen in explaining a series of experiments, which were instituted for the purpose of elucidating the nature of living action, cannot be too highly extolled,) imagines that the nervous influence always acts as a *stimulus* to muscular fibre; it is considered to be the sole stimulus of the voluntary, and the occasional stimulus of the involuntary, muscles. The involuntary muscles being incessant in their action, must, of course, be also stimulated into action by some *other* agent, whose influence is more permanent. This view of the subject very nearly corresponds with the old, and I think I may say now exploded theory, by which physiologists attempted to explain the action of the heart; and it seems to involve the same difficulties. As the blood which passes from the lungs to the left auricle and ventricle of the heart is scarlet, and that which returns from the other parts of the system to the right auricle and ventricle is black, it was supposed that *that substance* which produced the scarlet colour of the blood, and which it had apparently acquired in its passage through the lungs, was the appropriate stimulus of the left side of the heart; but the black colour of the blood was not attributed to any thing which the blood had *acquired* in its circulation through the system, but to the *loss* of that substance which it had previously acquired in its passage through the lungs, and therefore *the blood* ITSELF, which had lost this substance, was the appropriate stimulus of the right side of the heart. It seemed very improbable, and very unphilosophical, to imagine, that the two sides of the heart being similar in structure, function, and mode of action, should be obedient to the agency of stimuli so different, as the aërial influence (considered as a substance superadded to blood) and the blood itself. In the same manner, it appears to be very improbable, and very unphysiological, to imagine, that the voluntary and involuntary muscles, being similar in structure, function, and mode of action, should be obedient to stimuli, so different in their nature as the nervous influence and blood, or the nervous influence and alimentary matter. The theory, applied to the action of the heart, was rendered still more improbable by the circumstance, that, in the fœtus, both sides of the heart are obedient to the same stimulus; or, if there be any difference in the quality of the blood, that, which stimulates the *right* side of the heart, contains more of the aërial influence, or what is supposed to correspond with it, than the blood of the left side. In the same manner there is reason to suppose, that, in the fœtus, the action of the voluntary muscles is more dependent upon the arterial than upon the nervous influence. With what propriety can physiologists speak of the "purely vegetative life" of the fœtus, if its actions be dependent upon the *nervous* influence? It may be said, that I cannot, consistently with my theory, imagine the actions of the fœtus to be solely connected with the arterial influence, because every increased action, under these circumstances, implies an inflammatory disposition. But the fœtus is wholly removed from those causes which produce increased action; there does not appear to be any reason why it should take place. As, therefore, a secondary source of power does not appear to be necessary, I presume that it does not exist. Comparative physiology seems to point out a correspondence of action between the lungs and the nervous system. As therefore, in the fœtus, the lungs are quiescent, analogy would lead me to presume, that the nervous system is quiescent also.

The theory applied to the action of the heart was rendered still more improbable by the circumstance, that whole classes of animals possess, (if I may use the expression) a fœtal heart; that is, both sides of the heart are excited into action by blood of nearly, if not, under certain circumstances, of absolutely a similar quality. In the same manner, the action of the voluntary muscles, (which, according to the parallel I have drawn, correspond with the left side of the heart,) is supported by the arterial influence alone in the fœtus, in apoplexy, and in animals who have no brain or nervous system. Why should we imagine the existence of nerves, where they cannot be discovered, and the action of nerves, where there is every appearance of that action being suspended, if we can explain the phenomena of living action, under these circumstances, without imagining either? We learn, from the experiments of M. Le Gallois, that the action of the heart is almost immediately checked by piercing the spinal marrow; but that the surest mode of prolonging the life of an animal subjected to this operation is, having previously secured the large blood vessels so as to prevent death from hæmorrhage, *to cut off its head.* "Il est tres exact de dire que le plus sûr moyen de *faire vivre* des lapins de cét âge apres la destruction de la moëlle cervicale, c'est de commencer par leur *couper la tête.*"* What is the simple explanation of the fact? Having destroyed, or, at least, diminished the action of the nervous system, and consequently of the organs connected with it, we have diminished the expenditure of the arterial influence, upon which that action and those organs are dependent; and therefore, the supply of the arterial influence by artificial respiration is not so soon exhausted. Is not an animal *without a head*, the action of the heart still continuing, in a most perfect state of apoplexy? The author seems to have been aware that the experiment alluded to would be regarded as a strong proof of the action of the heart being independent, not only of the brain, but of the spinal marrow; and endeavours to guard against such an explanation of the experiment, by suggesting that the action which may be excited in the heart, after, not only the brain, but a considerable portion of the spinal marrow, have been destroyed, is nothing more than that action which may be excited in the heart, after it has been removed from the body. Après la destruction d'*une certaine etendue* de moëlle epinière, en quelque lieu de la colonne vertébrale qu'elle ait été faite, la vie ne continue *dans les parties dont la moëlle est restée intacte* qu'un temps déterminé et plus ou moins court, suivant l'âge de l'animal. Or, la durée de la vie, *dans ce cas*, se trouve être la même, que si le cœur eut été *arraché* dans un animal de même espêce, et de même âge."* The principal object of the author's experiments is to prove, that the action of the heart is connected with the spinal marrow as a whole; that is, with every individual portion of the spinal marrow. "Le cœur emprunte toutes ses forces de ce même principe, de même que les autres parties en empruntent le sentiment et le mouvement dont elle sont douées *avec cette difference*, que le cœur emprunte ses forces *de tous les points* de la moëlle *sans exception*, tandis que chaque partie du corps n'est animée que par *une* portion de cette moëlle."* And yet the author says, if the spinal marrow be destroyed to a certain extent, that is, if a certain portion only of the spinal marrow be left to influence the action of the heart, that action is nothing more than what takes place in the heart when it is actually removed from the body. "La durée de la vie, dans ce cas, se trouve etre la même, que si le cœur eut été arraché." What proof, then, have we, that the portion of the spinal marrow, which remains, exerts *any* influence over the life of the heart? Are we not told, in so many words, that it does not? If, then, the life of the heart be admitted to be uninfluenced by a small portion of the spinal marrow, that is, when the spinal marrow has been destroyed *to a certain extent*, what is the precise point of perfection or imperfection of the spinal marrow, at which the life of the heart is, or is not, influenced by it? in other words, *how much* of the spinal marrow may be destroyed, before we must consider the life of the heart to be nothing more than that which it possesses when removed from the body, and when it must be wholly independent of the nervous system. Is it not reasonable to suppose, that this is the precise state of the heart, as far as the *nervous* influence is concerned, when the head has been cut off? The same author records an experiment, (at the bare mention of which humanity shudders,) in which the several parts of the animal were successively detached, until nothing but the thorax, the thoracic viscera, the corresponding portion of the spine, and the *anterior extremities*, were remaining. Artificial respiration was then had recourse to; and what are the proofs that life, such as it was, still continued. "Les plus apparens de ces signes sont *les mouvemens et la sensibilité* que conservent *les pattes anterieures*, et les petits mouvemens de torsion, que fait *le thorax* quand on pince fortement le peau, et surtout, quand on touche l'extremité posterieure de la moëlle dorsale."* Consistently with the author's opinion, already adverted to, the action of the heart,

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under these circumstances, must be similar to that which may be excited, when that organ is removed from the body. If the heart be uninfluenced by the small portion of spinal marrow remaining, and its action be supported by the arterial influence alone, is it not probable, that in the mutilated state of the spinal marrow, the action of the voluntary muscles was supported in the same manner? Is it not probable, that this was nothing more than a galvanic experiment, and that the action excited has no nearer connection with the *life* of the animal, than that which is ordinarily produced through the medium of this influence? If respiration be a process by which oxygenated electricity is separated from the atmosphere, have we any difficulty in accounting for the phenomena observed? I am by no means clear, that this view of the subject does not apply to *all* those experiments, by which it has hastily been inferred, that an animal continues *to live* after the brain and spinal marrow have been destroyed, and after it has been deprived of its "fair proportions," and reduced to a moiety of that form, which nature has declared to be alone consistent with the life of the animal. If the similarity between the galvanic and arterial influences be admitted, and the similarity of the actions excited by the former, and those excited by artificial respiration, after the brain and spinal marrow have been destroyed, be duly considered, I am inclined to suppose that some little check would be given to that enthusiasm, which could lead an author to indulge in so visionary an idea, that he not only had actually produced resuscitation of parts, *that were dead*, but that, if the same means could be applied to the body in general, the whole body might be re-animated. " C'est donc la cessation de cette impression, c'est l'extraction de ce principe qui constitue la mort; et par consequent, pour faire succeder *la vie à la mort*, ou, en d'autres termes, *pour opérer une resurrection*, il faudroit renonveler ce principe."* " Mais s'il existoit quelque moyen de suppléer à la circulation naturelle, qu'il n'est plus possible de ranimer, il est certain que l'on pourroit *rescusiter un cadavre après la mort;* temps que seroit limité par plusieurs circonstances, et variable suivant l'espéce, l'âge de l'animal, les causes de sa mort, les saisons, &c. *Les resurrections partielles que l'on peu opérer à la volonté, ne laissent aucun doute à cet egard.*"† Dr. Wilson Philip says, " It may here be objected, that in apoplexy the power of the muscles of voluntary motion is lost, while that of the heart is little, if at all impaired. Were such the fact, this objection would be unanswerable; but I have repeatedly examined the state of the muscles of voluntary motion in apoplexy, both in the warm and in the cold-blooded animals, and found their excitability unimpaired. It is not their power, but the *stimulus* which excites them, that is lost in apoplexy."* How does this view of the subject correspond with the following passage:—" We know the peculiar office of the brain, by observing what functions are lost by its removal—the sensorial functions. The nervous, then, obeys the sensorial system, in the same way in which the muscular obeys the nervous system; but as the *muscular* system has an existence *independent* of the nervous, so has the nervous, independent of the sensorial system."† In the word existence, the author evidently means to include function. How can the function of the muscular be independent of the nervous system, if the nervous influence be the *sole* stimulus of involuntary muscles? The author adds, " what is here said, is finely illustrated by reviewing the various classes of animals. In the lower class, we find *only* the muscular, which exists without *either ner-*

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* Experiences. † Ibidem.

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* Philosoph. Transact. † Ibidem.

vous system or sensorium."* Is it not *assuming* a fact, to say, that these muscles are all involuntary? Is it not *assuming* a fact to say, that they *must be* involuntary, because there is no brain? But the author rests his opinion upon an argument which he considers to be decisive. The nervous system acts as a stimulus to voluntary muscles, because it is a source of exhaustion and not of power. If this fact were established, the argument would be unanswerable. The analogy between the nervous and galvanic influences (which analogy has been particularly suggested, I may say, confirmed by the experiments of this author) would have led us to anticipate a directly opposite conclusion. "The most important and characteristic difference, which, Dr. Fowler observes, he has been able to discover between this new influence and electricity, consists in their effects upon the contractile power of animals and of plants. He has found, that *so far from producing exhaustion, the more frequently* contractions have been excited by this influence, *the longer* do they continue excitable."† According to the experiments of Dr. Valli, "Animals with warm blood present us with different phenomena. The wings of fowls left prepared for a quarter of an hour, he observes, have only given a few slight tremblings. If, however, their electricity be excited immediately after they are prepared, the movements last an hour or even longer. Does this excitement support life, instead of destroying it? In a dog, while one of the fore legs gave strong shocks, the other, hitherto untouched, was prepared, but did not exhibit the least motion."* "Reinhold has, as well as many other physiologists, been convinced, that galvanism itself *augments* the incitability of animals. After several *feeble* movements, he frequently found that others of a *powerful* nature supervened in a very unexpected way. He observes that they cannot, according to any strict mode of reasoning, be ascribed to a state of repose, experience having *demonstrated*, that the galvanized thigh of a frog, or other animal, displays weaker movements in proportion as it has remained longer in a state of repose and without incitability."† Dr. Philip having almost proved the identity of the nervous and galvanic influences, by exciting the digestive action of the stomach through the medium of the latter, is of opinion, that nervous influence exhausts, instead of adding to, the irritability of muscular fibre. "From this experiment (he says) it is evident, that the nervous influence, so far from having a power of preserving the excitability of the muscles, exhausts it like other stimuli."* The following is the experiment recorded:—"All the nerves supplying *one* of the hind limbs of a frog were divided, so that it became completely paralytic. The skin was removed from the muscles of the leg, and salt sprinkled upon them, which, being renewed from time to time, excited contractions in them for *twelve* minutes; at the end of this time, they were found no farther capable of being excited. The corresponding muscles of the other limb, in which the nerves were entire, and of which consequently the animal had a perfect command, were *then* laid bare, and the salt applied to them in the same way. In *ten* minutes they ceased to produce any contractions, and the animal had lost the command of them. The nerves of this limb were now divided, but the excitability of the muscles, to which the salt had been applied, was gone. Its application excited no contraction in them."† The muscles then, the nerves of which had been divided at the commencement of the experiment, lost their irritability in *twelve* minutes; but those, in which the nerves were not divided, retained their irritability for ten minutes only. But what was going on in that limb, which was the object of the second experiment, during the *first* twelve minutes, whilst the other limb was having its nerves divided, was being skinned, and salted ? Were there no struggles, no convulsive actions of the system, by which its irritability might have been diminished? Was the animal free from pain during such an operation ? With what consistency can the author imagine, that the irritability of the limb, which was not operated upon, remained precisely the same and unaltered, when he himself says, " Such is the power of the sensorium over the nervous system, that its affections may, through this system, at once destroy *every function of life*."* It is clear that the conclusion deduced from this experiment is wholly unwarranted, unless it can be shewn, or, at least rendered probable, that, at the commencement of each experiment, the irritability of each limb was precisely the same. The *power* of the contractions also, the comparative rapidity with which the muscles, under these different circumstances, may lose their irritability, and the difference in the state of disease of the two limbs, are manifest sources of error, and render the ex-

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* Philosoph. Transact. † Med. Comment. 1793.

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* Med. Comment. 1793.

† Wilkinson's Galvanism.

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* Philosoph. Transact. 1815.

† Ibidem.

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* Philosoph. Transact. 1815.

periment very far from decisive; and, supposing these difficulties to be removed, does it follow, as a matter of course, that the nervous influence must affect a muscle which has been skinned and salted, and a healthy muscle, which has been subjected to neither of these operations, in the same manner? The theory appears to be unphysiological, because it attributes the same action in the same organ (I am now alluding to the *occasional* influence which the nerves are supposed to exert over the *involuntary* muscles) to different causes; and it is unsatisfactory, because the advantage which the involuntary muscles thus derive from their nerves, is not very apparent. I am of opinion that the theory, which connects the action of the heart with the arterial influence, by supposing that the latter unites with the structure of that organ, and renders it *susceptible* of action, is the most satisfactory which has been offered. In the same manner, I imagine that the nervous system has but one function to perform, viz. to communicate that influence, which it has derived from the blood, and which is nothing more than the arterial influence, to the several parts of the system, according to their necessities; and I imagine that the nervous corresponds with the arterial influence, in contributing to the support of living action, not by acting as a stimulus to, but by uniting with, the structure of an organ, by which that organ acquires susceptibility of action only.

If to an organization, which we denominate vital, and which may be so denominated without possessing susceptibility of action, moisture, heat, and the arterial influence be added, in those peculiar proportions and degrees which nature has assigned to each individual organized being, that organization acquires the capability of being excited into action. If to this organization an appropriate stimulus be applied, action is always the consequence. The *immediate* stimulus of every organ is a certain change in the relative situation of its component particles, and this change may be the effect either of mechanical or chemical agents; the action of every organ is the return of these particles to their natural situation, and the power of an organ is that property upon which this return or re-action depends; so that the power and the life of an organ appear, at first sight, to be the same things. We have said that the *life* of an organ is always the same; that there is no medium between positive life and absolute death; that an organ is either living or dead. But the *power* of an organ is constantly varying. What, then, is the difference between the vital principle and the vital energy of an organ? This question may probably be answered, by adverting to the opinions of Dr. Adair Crawford upon the cohesion of animal fibre. " Dr. Crawford begins by explaining the exact meaning which he attaches to the term cohesion, by which he proposes to express not only what is more strictly entitled to this appellation, but likewise the qualities which are usually denominated firmness, elasticity, and strength." These qualities are thus described:—" By the firmness of the fibre, I mean to express the force with which it resists impressions; by the elasticity, its power of resisting extension, and of restoring itself, when the extending cause is removed; by its strength, the force which it is capable of exciting in opposition to such causes as tend to destroy the continuity of its parts. Besides these properties, which belong to *dead* matter, we have *others*, which are exclusively inherent in the *living* fibre, of which the most important is contractility, or the power which the particles possess of approximating themselves. Upon the *union* of *this* quality with *that* of elasticity, it is conceived, that what is styled the tone of the fibres depend."* So that life is a property acting upon matter, but independent of it; power is the union of this property with that of matter itself—the former being superadded to, and being independent of, matter, is always the same; the latter will vary with the state of the matter itself. If, then, to an organization denominated vital, moisture, heat, and oxygen, either in the form of the arterial or nervous influence, be added, that organization acquires the *power* of being excited into action. If to an organization possessing this power, a cause which changes the relative situation of the organic particles, be applied, these particles will contract upon themselves, and this contraction constitutes action. But the difference between voluntary and involuntary muscles is this, that the former are *naturally* in a state of tension, and therefore possess the *stimulus* to action in themselves; the latter do not possess the necessary degree of tension, and are, therefore, dependent upon some other cause to produce it.

Living action, then, is motion; but the *effect* of living motion is mechanical, as far as the moveable solids are concerned; is mechanical and chemical, as far as the fluids are concerned; but is neither mechanical nor chemical, but unintelligible—may I say *unimaginable?*—when we endeavour to trace the connection between mind and matter. But the action of organized matter not only produces certain changes in *other* solids and fluids, and, in an unknown manner, upon the sentient principle, when subjected to its influence, but also in the organization itself. The particles gradually lose their vitality, and are taken up by the lymphatic absorbents, for the purpose of being thrown off by the different excretions. Thus in the carbonic acid perspired and expired, we recognize the particles of animal matter, which have performed their functions, which have contributed to the support of living action, and having lost their vitality, are become excrementitious. If we go still further back, that is, to the sources of organization, we recognize in the carbonic acid excreted, the carbon of our food, and the oxygen of the atmosphere; and it is probable that, generally speaking, a considerable portion of the other *effete* particles are contained in the other excretions. Much important and practical information would accrue from our being able to determine by what peculiar excretion the *different* particles of organized matter are eliminated from the system. This, I fear, it will be difficult to ascertain, but I shall offer some few observations upon the subject in its proper place. The connection between living action and animal heat is very apparent. I am disposed to regard animal heat as, in reality, a secretion. The consideration of this subject will be introduced, with more propriety, in a subsequent part of these observations. But considering animal heat as the effect of living action generally, it appears to be intimately connected with the remarks contained in this chapter. I shall, therefore, shortly express what are my general views upon this subject.

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* Ed. Med. and Surg. Journ. No. 51.

The phenomena of torpid animals are, of themselves, sufficient to establish the fact, that a most intimate connection exists between living *action*, and the development of animal heat. They also establish another fact of equal importance, that there is no necessary connection between animal heat and life, because we find the characteristic property of life (viz. the preservation of animal matter under circumstances which, but for the agency of this principle, would decompose it) to be *equally* apparent, whether the temperature of the animal be at 30 or 97. " Dr. Jenner, whose discovery of vaccination has conferred so much honour on himself, and such incalculable benefits on mankind, at the request of Mr. Hunter, examined the temperature of a hedgehog, in its torpid state. In an atmosphere of 44°, its temperature was, in the pelvis, but 45°, and at the diaphragm 48. 5°; and when the atmosphere was reduced to 26°, the heat of the hedgehog *in* the abdomen did not exceed 30°, but in summer, when the air is at 78°, and the animal in an *active* state, its temperature rises to 95° in the pelvis, and to 97° at the diaphragm."* It may be said, that consistently with my own views, viz. that *no* action is going on in torpid animals, there ought to be *no* animal heat, not even the difference between 44° and 45°; but how shall we *get at* the pelvis, the diaphragm, and the internal part of the abdomen, without *exciting action*? I have given my reasons for supposing, that the arterial and nervous influences are the same things, and that they are a peculiar modification of oxygen with electricity. We may imagine electricity and caloric to be distinct things, but we can form no idea of electricity, which does not include the idea of caloric. We are under the necessity of admitting, that electricity, if not caloric itself, must, at least, be some modification of that principle. The nervous and arterial influences, then, may be regarded as a compound of oxygen with *heat* in a peculiar state of modification. We not only find that a due supply of oxygen is essential to the existence of living action, but that oxygen does not contribute to the support of living action, unless it be itself acted upon by a living system. If we trace the oxygen one step further, we not only find that a due supply of the arterial or nervous influences (compounds of oxygen with *heat* in a peculiar state of modification) is essential to the existence and the support of living action ; but that the supply of these influences is regulated by the action of the circulating and nervous systems. We find, that the action of the circulating system is not only to convey blood to the several parts of the body, but to decompose it. Analogy, therefore would lead us to suppose, that the arterial influence is also decomposed. We have here, then, an evident source of animal heat, even in those animals who have no nervous system. The arterial influence (a compound of oxygen with heat in a peculiar state of modification) is decomposed. Oxygen unites with the organ, and contributes to the support of its irritability, and the heat is separated. In proportion, therefore, to the action excited, that is, in proportion to the quantity of arterial influence decomposed, will be the quantity of heat evolved. In the more perfect animals, the nervous system is a secondary source of the arterial influence. Analogy would lead us to suppose, that the action of the nervous system is not only to convey the arterial influence to the several parts of the body, but to decompose it; and the analogy is, in a remarkable manner, preserved by the circumstance, that this secreting action is accomplished by the *extremities* of nerves and of arteries. The remarkable correspondence between the development of the respiratory organs and of the nervous system, and the degree of heat which is natural to the different classes of animals, will be adverted to hereafter; but this general view of the subject is sufficient for our present purpose, viz. that of pointing out the connection between living *action* and animal heat.

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* Ellis.

CHAPTER XIV.

LOCOMOTIVE FUNCTIONS

PREVIOUS to the time of Haller, and, indeed, during the early part of his life, the connection between the influence of the brain, and the action of muscular fibre, was almost universally admitted. So intimate appeared to be this union, that muscular fibres were considered to be the continuations, the *moving extremities* of the nerves themselves. Haller first doubted this connection, and finally denied it altogether. It may not be uninteresting, or unimportant, to observe the remarkable change which took place in the opinions of this justly celebrated and eminent physiologist. In a translated edition of his lectures on physiology, we meet with the following passages:—"But that the cause of motion is conveyed through the nerves into the muscles is *certain*, from the experiments before mentioned."* "These things being considered, there seems to be *no doubt* but the cause of *all* motion in the human body arises from the brain, with its annexed cerebellum and spinal marrow, and that it thence proceeds through the nerves to *all* the muscles and moveable solids of the body. The cause therefore, of this motion *cannot* reside or dwell in the parts themselves, because, otherwise, the moving cause would continue to act after being separated from the brain, nor would it be increased by irritating the brain, or weakened by a compression of it."* The author subsequently found, that the heart *was* capable of being stimulated into action after it was removed from the body; but he did *not* find " that its action could not be increased by irritating the brain, or weakened by a compression of it;" and to this oversight of what was essential to the support of his theory may be traced all those difficulties with which it has had to contend, and which, in my opinion, are absolutely irreconcileable, and will ever continue so, with the doctrine of Haller.

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* Haller's Physiology, 1754.

It may not be uninteresting to direct our attention to the growth of these opinions, and to observe in what manner the reasonings upon a few insulated facts were moulded into the dimensions and form of a general theory.

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* Haller's Physiology, 1754.

The dependence of the heart upon the brain was not questioned, until certain *pathological* facts occurred, which were inconsistent with the prevailing opinions of the day. A new theory was immediately suggested, by which the explanation of these insulated facts was facilitated, but which was in direct opposition to other facts equally important and undeniable. In the first place, the inference upon which the theory is built, (for it will be found to emanate from the *explanation* of the fact, and not from the fact itself) is very questionable. Certain phenomena present themselves to our observation, when the function of an organ is deranged, and *therefore*, they occur when this function is carried on with the regularity of health. Is such an inference to be received as evidence? It might almost as well be said, that the arteries secrete pus in health, because they are found to do so in disease. It certainly amounts to nothing more than probability. Let us, then, without adverting to those phenomena which present themselves in disease, consider how far this *probability* is corroborated by the phenomena of health? I think it *probable*, that the action of the heart, in health, is wholly independent of its nerves, because I find that, in disease, that organ is capable of acting without them. The heart, it must be admitted, is supplied with nerves, but I cannot find out any use for them, and therefore, consistently with my theory, I think it probable, *that they are of no use at all;* or, I admit that they must be of some use, and that they influence the action of *other* organs; but I suppose that the nerves of voluntary and involuntary muscles have *different* functions to perform; what that difference is I am unable to say, but it is of no consequence, because I can explain the action of the heart without them; or, it is quite a mistaken notion that the heart is supplied with nerves; they are merely distributed to the neighbouring organs. I admit that Haller, who was one of the best and most indefatigable anatomists, and who was particularly interested in the discovery of this fact, says, " these fibres of the heart are, like other muscles, furnished with nerves of their own, *very numerous*, and of various origin; these nerves descend *into* the heart, partly on each side the aorta, betwixt that vessel and the pericardium, and are distributed all over the surface;"* but he was mistaken; *aliquando dormitat Homerus.* Scarpa is a most excellent anatomist, and maintains the same opinion, but he must be mistaken; it is not consistent with my theory. I can explain the action of the heart without nerves, and therefore the heart cannot be supplied with them; or, perhaps, in a moment of irritation, the truth might slip out; I wish, by my faith, there were no cardiac nerves; mine is a beautiful theory without them.

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* Haller's Physiology, 1754.

The theory is not founded upon the fact itself, but upon an imaginary fact supposed to be connected with it. When the heart is removed from all communication with its nerves, must it necessarily be deprived of that influence which it had *previously* acquired? The only fact established by the experiment is this; the heart is capable of being stimulated into action, *without a constant supply* either of the arterial or nervous influences.

The next inference is equally unwarrantable; the heart is *capable* of acting independently of the nervous influence, and therefore it always does so. The voluntary muscles are known to act in sleep, and often in opposition to the will; does it therefore follow, that they never act in obedience to it? The doctrine of Haller, in its full extent, is, I conceive, wholly untenable; and the great difficulty to be overcome appears to be that of restoring to its nerves a function adequate to their importance in other parts of the system, and not differing from the operation of nerves in general.

An important modification of this doctrine, as proposed by Dr. Wilson Philip, has already been adverted to. I agree with that author in supposing, that the action of the heart is principally dependent upon the arterial influence, and, under some circumstances, I believe it to be dependent upon the arterial influence alone. But I do not believe that this is the *only source of power*; because it is inconsistent with all analogy to suppose, that the power of any organ is solely dependent upon itself; because it involves the contradiction, that an organ loses and recovers power by the same action; because it is inconsistent with all reason to suppose, that an organ should be called upon to exhaust itself, as the only means of restoring its power; because we have every proof, as far as our senses will enable us to judge, that under certain circumstances, the action of the heart does positively cease, and is again *renewed*, which could not possibly take place, if the supply of power were dependent upon the heart alone; because it involves the contradiction that the heart must act, before it acquires the power of doing so; and because the increased *action* of an organ already in a state of debility, would be a prolific source of disease. These circumstances lead me to conclude that there must be a secondary source of power.

I agree with the same author in supposing that the nerves impart an occasional influence to the involuntary muscles; but I believe this to be the action of nerves in general, and that this influence is imparted to the involuntary muscles, according to their necessities, and to the voluntary muscles according to the necessities and the will of the animal. I agree with the same author in supposing that the nervous influence is a source of action, but I do not attribute this action to any *stimulant* effect of the nervous influence; because I do not admit that the same action, in the same organ, is, in the living system, ever produced by different causes; and because I do not see what possible advantage an organ can derive from this secondary stimulus, from this secondary cause of exhaustion. If the nervous and arterial influences be the same things,—(and that they are so I conclude from analogy, from observation, and from experiment, from the circumstances that no one has yet pointed out in what respect they differ, and that we cannot suppose them to be different, without attributing to the brain a secreting action which is inconsistent with its organization),—if, I say, the nervous and arterial influences be the same things, and if the latter be a source of power to an organ, do we not, in the nervous influence, recognize that secondary source of power, which seems to be *necessary* under certain circumstances, both to the action and to the health of the system? Do we not reconcile the facts, that the action of the heart is, under certain circumstances, wholly independent of the nervous influence, and at other times solely dependent upon it? Do we not attribute to the cardiac nerves a function, adequate to the importance, and similar to the operation of nerves in general?

Irritability was considered by Baron Haller to be a property, a *vis insita*, peculiar to muscular fibre. It is wholly unnecessary, in the present day, to revive the celebrated controversy between that physiologist and Dr. Whytt, as to the cause of irritability; that is, whether it is to be regarded as an inherent property of muscular fibre dependent upon its gluten, or a quality dependent upon the nervous influence. Modern experiments have established an intimate connection between this property and the supply of oxygen. It has been supposed, that oxygen is the source of heat, and that this heat is the cause of irritability. Thus Mr. Coleman says, " we scarcely know of any animal, on whose blood the air does not produce some change, either directly or indirectly ; and the great object of this change we deem to be the support of animal heat ; and from the maintenance of animal heat that of animal irritability."* But heat and oxygen are both necessary to the irritability of an organ. The theory proposed by Bichât, is that which, of late years, has been most generally adopted. The arterial influence unites with the structure of animal fibre, and the result of this union is that property denominated irritability, or susceptibility of action. If the nervous and arterial influences be the same things, we must suppose that their effect is also the same. The nervous influence, therefore, uniting with muscular fibre, will contribute to the support of its irritability. Every organ, which has an action to perform, must possess susceptibility of action, and this appears to be precisely the same, whether it be derived from the arterial or nervous influence. Irritability, therefore, is not a property of muscular fibre alone, but of every organ which has an active function to perform.*

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* On Suspended Respiration.

"But, before entering upon this discussion, it will be necessary (says Dr. Girtauner) to prove, that the irritable fibre is universally diffused through all organized bodies. Its existence is known and generally admitted in animals with warm blood, and in some that have cold blood, such as frogs, lizards, tortoises, eels, serpents, and fishes. Insects, worms, and plants, are not less endowed with irritability. The irritability of oysters, of medusæ, and of polypi, is well known."† The existence of irritability in the vegetable kingdom has been proved by experiments upon a great variety of plants. The fact is as well established by the climbing growth of the garden pea or of the hop, as by the experiments of Fontana, Hales, Du Hamel, Des Fontaines, Bonnet, Ingenhouz, and Hunter. It has been said, that the common hop will *untwist* itself from a pole which has become rotten and decayed, and creep along the ground in search of another support. This property of vegetables is alluded to by Linnæus. "Gratiola, œstro venereo agitata, pistillum stigmati hiat, rapacis instar draconis nil nisi masculinum pulverem affectans, et satiata rictum claudit." The irritability of *plants* may be traced to the same source as that of animals, viz. the oxygen of the air. If to a certain organization, which we denominate vital, moisture, heat, and oxygen be added, that organization, whether animal or vegetable, acquires susceptibility of action. In plants, one source of exhaustion (the influence of the mind) is wholly wanting; the motions are limited; the supply of oxygen, through the medium of the leaves, is immediately and generally diffused through the system; and the property is durable, as is proved by the circumstance of boughs continuing to bud, and even to expand their leaves, after they have been separated from the parent tree. No secondary source of power, therefore, is necessary in vegetables, and, consequently, they have no nervous system.

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* The bones have a function to perform which can scarcely be denominated a living action.

† Med. Comment. 1790.

These observations apply equally to *zoophytes*, in which no nervous system is apparent, with the exception, that something like voluntarity must be attributed to these animals. Professor Cuvier, in speaking of the echinus, says, "it is very difficult to discover the fibres by which the spines are moved *at the will* of the animal."* I do not conceive that the imperfect act of voluntarity, here alluded to, necessarily implies the agency of nerves.

As no secondary source of power appears to be required, I think it unnecessary to *imagine* the existence of nerves in zoophytes. In worms, we find a very imperfect nervous and circulating system, and stigmata instead of lungs. But their motions are sluggish, and the slow expenditure of oxygen is one of their characteristic properties. "Worms have the faculty of surviving long in impure air."† A secondary source of power must be comparatively unnecessary, and therefore a very imperfect nervous system is sufficient to answer the demands of the animal. In *insects* we begin to discover something like brain—"two little medullary bodies instead of brain."‡ The circulating system is, comparatively, more perfect, and we meet with tracheæ or branchiæ, instead of stigmata. But we still find that the expenditure of oxygen is slow. "They possess the faculty of surviving long in impure air." An imperfect nervous system is sufficient to answer the occasional demand which is made upon it; but the nervous system of zoophytes and of worms would be insufficient, for we find in insects evident causes of occasional expenditure. "The great variety of motions which insects are capable of performing, entitles them to hold the first rank amongst the animals that have no vertebræ. We discover, in these minute beings, all the requisites necessary to produce those voluntary actions, the execution of which astonishes us in vertebral animals of far greater magnitude. They even unite several functions which are very seldom found combined in the other classes; for insects walk, run, leap, swim, and fly, with as much facility as the mammalia, birds, and fishes, exercise one or more of these faculties."* The imperfect nervous system already described, might appear to be insufficient for the support of such multiplied actions; but these actions are, generally speaking, uniform and regular; they are attended with a small expenditure of oxygen; and nature has provided for an additional supply of this principle, through some other medium than the nerves. There are supernumerary lungs, which are exposed to the atmosphere at that very time, when the increased exertions of the animal require an increased supply of oxygen. "Amongst insects, we find that many of the coleopterous species disclose avenues of air in the act of flying, which in their quiet state are closed by the cases of their wings, thus procuring for themselves a larger supply of oxygen, at a period when, from their exertions, they most require it."* Admitting, as we must, the evident connection between the supply of the arterial or nervous influences, and the susceptibility of action, if the same connection be pointed out between the supply of oxygen and the capability of motion; is it not reasonable to suppose, that the effect being the same, the cause is also the same? If a muscle be rendered paralytic by the exclusion either of the arterial influence, of the nervous influence, or of oxygen, is it not probable that the paralytic state of the muscle is dependent upon the same cause? Insects afford us a remarkable opportunity of exemplifying this fact, because oxygen may be partially excluded, without interrupting the general action of the system, which cannot be done in the superior animals; and because the muscular system is sufficiently developed to enable us to observe the effect of our experiment, which is not the case in the inferior animals. Dr. Kellie says, "It is well known how easily insects are destroyed, or rendered torpid, by closing their stigmata with oil. In caterpillars we can, in this way, experiment very instructively. Close the whole of the stigmata, and you kill the animal; the stigmata of one side only, you induce hemiplegia; one half the stigmata on both sides, paraplegia is the consequence."*

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* **Comparative Anatomy.** † **Dumas.** ‡ **Ibidem.**

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* Comparative Anatomy.

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* Dr. Paris.

In fish and reptiles, the nervous system approximates to that which is found in the higher classes of animals. In the former it is extremely small. The brain and the medulla spinalis of the squalus maximus is exhibited in the Museum. It therefore appears that fish, such *agile*, *powerful*, *indefatigable* creatures, *which are also so surprisingly retentive of life*, have not only a very languid circulation, scanty respiratory process, but also a disproportionately small nervous system."† *But the nerves are large;* and nature seems to have been so anxious, that oxygen, in its purest state, should be conveyed to the organs of locomotion, that the arterial blood does not return from the branchiæ to the heart, by which an unnecessary delay would be occasioned, *but is immediately* distributed through the system; to those organs which have the most laborious function to perform.

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[*] Ed. Med. and Surg. Journ.

[†] Abernethy's Lectures.

If we look to the slow contraction of the heart, and the little power which can be necessary merely to impel the blood to the branchiæ, (for this is the sole action of the heart in fishes,) and compare them with the rapid and powerful contractions of other muscles, and the great power which must be required to impel the whole body through so dense a medium as water, the connection between the supply of oxygen and the degree of living action, so far from being disproved, is strikingly exemplified. The nerves are large, and therefore the secondary source of power is most developed in those parts of the system where it is most required.

In *amphibious* animals, and in fishes, the nervous systems very nearly correspond; but there is a marked difference in the structure of their circulating systems. The peculiarity of this structure in the former, is more calculated to counteract than to promote the oxygenation of the blood; the blood is industriously diverted from the lungs, in order that it may circulate through the system in a less oxygenated state. In the latter, the supply of oxygen is so essential to the system, that the whole of the blood is sent to the branchiæ, and is immediately (without returning to the heart, as in the mammalia) distributed to the several parts of the body. The double life of amphibious animals would seem to require two modes of respiration; lungs for the air, and branchiæ for the water. By what a simple contrivance has nature provided for this difficulty, and given to these animals the power of living in both elements. There is a store of oxygen laid up in the system; they live in the water without respiration; they may be almost said to respire the nervous influence. Does not nature tell us that this oxygen is not derived from the blood? The blood is imperfectly oxygenated: why? Because, if in the air the actions of the system were attended with a rapid expenditure of oxygen, the same thing must occur in the water, which would be inconsistent with the continuance of life independently of respiration. The nervous system is more fully developed in the mammalia, and therefore, it may be said, these animals ought to have the power of living without respiration, even longer than those of the amphibious class. If we consider that, in the amphibia "the nerves are large, extended, strong;"* the fact, as to the comparative development of the nervous system may be questioned. But if we look to the marked distinction between the mammalia and the amphibia, we shall at once see why the latter can, and the former cannot, live for a considerable time without respiration. According to Dumas, Vicq, d'Azyr, and others, the respiration of the mammalia is "frequent, extended, regulated, requiring pure and oxygenated air;" that of the amphibia is "unfrequent, unequal, and irregular," and these animals possess "the aptitude to respire long in every species of air;" in other words, the mammalia require a constant supply of oxygen, and the amphibia do not. The supply of oxygen, except under particular and transient circumstances, must always indicate the expenditure. The marked distinction, then, between the mammalia and the amphibia is, that the actions of the former are attended with a greater expenditure of oxygen than those of the latter. From the acknowledged connection, then, between the supply of oxygen and the irritability of muscular fibre, it follows that the mammalia expend their irritability faster than the amphibia, and this we find to be, in reality, the case. The irritability of the latter is described as durable and existing long after death—(apparent ?) Nature, therefore, by rendering the irritability of the amphibia less exhaustible (if I may use the expression,) has rendered them less dependent upon a constant supply of oxygen. The more perfect development of the circulating and respiratory organs, such as we observe in the mammalia, would be inconsistent with such a state of the system; and therefore we find a fœtal heart, and the air cells of the lungs large, by which the extent of surface exposed to the atmosphere is diminished. Still, however, it may be said that in fishes, and in amphibious animals, the expenditure of oxygen, if it correspond with the actions of the system, appears to be disproportioned to the supply. But the expenditure of oxygen is also proportioned to the state of the muscular fibre. Thus a well-trained pugilist will support a certain degree of action with a less expenditure of oxygen than the nervous hypochondriac, and is said to be *in good wind;* that is, the expenditure of oxygen being less, the necessity of its supply through the medium of the lungs is diminished. There may, then, be a peculiarity in the muscular fibre of fishes and amphibious animals, by which they expend less oxygen; and such appears to be the case in amphibious animals, for we are told, "their contractions and dilatations are *strong*, but slow; their muscular power immense." I think it by no means improbable, that this peculiarity may exist in different organs of the human body. Thus the strength of the heart may enable it to support a certain degree of action with a comparatively small expenditure of oxygen. We see the advantage which that important muscle would derive from such a circumstance.

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* Dumas.

In the *mammalia*, we find the most perfect respiratory organs, a circulation consisting of a double heart, by which the whole of the blood is sent to the lungs before it is allowed to circulate through the body, and a nervous system more perfectly developed than in any other class of animals. I have already attempted to shew, that we cannot satisfactorily account for the phenomena of living action, such as they occur in man, consistently with the idea, that the human body never derives oxygen from any other source than the blood, and that a secondary source of power is, in the mammalia, a necessary provision of nature. Why should this secondary source of power be in a peculiar manner necessary to the human system? Man is, more than any other animal, exposed to those circumstances which require varied and irregular exertion, and the exertion occasionally required is, in a peculiar manner, disproportioned to his physical powers. The influence of his passions is a never-ending source of varied and irregular action; and thus he is more particularly exposed to those circumstances which excite and depress the energies of the system; and these occur not at stated periods, not in consequence of any fixed and irregular exertions, such as are necessary to the support and life of other animals, but as the results of his disposition, feelings, and passions. He is in, a peculiar manner, exposed to the exciting and depressing effects of disease and of climate.

The preservation of life, as far as the corporeal part of our frame is concerned, seems to be the first and great object of that power which first called it into existence. The infant, deprived of that *unerring* parental care which, in other animals is the impulse of instinct, that is, the voice of nature speaking through her works; exposed to all the dangers which arise from over-anxiety on the one hand, and neglect on the other; and, at best, dependent upon the wavering impulse of affection, or the erring dictates of an imperfect judgment; the infant, I say, thus circumstanced, is more particularly exposed to causes of exhaustion, and is, in a peculiar manner, endowed with a secondary source of power in the nervous system. If we consider the nervous system to be the organ of sensation alone, what possible advantage can the infant, already sensitive to excess, and not unfrequently the victim of disease in consequence, be supposed to derive from this energetic state of the nervous system? Do we not, in reality, find, that the organs of sensation are comparatively useless, imperfect in their action, and unimportant in their effect? If we consider the nervous influence to be a *stimulus* to muscular fibre, what possible advantage can the infant, irritable to excess, convulsed by the slightest causes, and unable to direct this stimulus to the excitement of voluntary motion, be supposed to derive from such a state of the nervous system? If we consider the nervous system to be the *only* source of power, how shall we reconcile this fact with the irritability of plants, the growth of the egg, the living action of the zoophyte, the growth of acephalous fœtuses, the continuance of action in apoplexy, the recovery of action in syncope, and with all those experiments which have induced physiologists to imagine, that the action of the heart is wholly independent of the brain? We may deny the analogy in this respect between animals and plants, whilst, with marked inconsistency, we admit it in others. We may *ima-*

gine that, in apoplexy, the voluntary nerves are affected, whilst the involuntary continue to perform their functions with unabated energy; and we may imagine this to be the case, although we have never satisfactorily pointed out any distinction between the nerves of voluntary and involuntary motion, *sufficient to justify us in saying*, that they are not precisely the same; and although we admit, in other cases, that there is a manifest sympathy between them. We may admit that the action of the brain is dependent upon the influence of arterial blood, and that the heart supplies to itself its own power of action through the medium of the nervous system, and that, in syncope, the action of the heart is debilitated or suspended, in consequence of the nervous influence being interrupted; and in order to account for the power of the heart being restored, we may imagine that it acts before it has acquired the power of doing so; that it recovers power by the same action which, under other circumstances, exhausts it; or that the brain may act without the *stimulus* of that arterial influence which we have before admitted to be essential to its action. Are not these difficulties obviated, by our regarding the nervous and arterial influences as the same things?

The analogy between the fœtus and the amphibious animal is very striking, whether we regard the different elements (if I may apply that expression to the fœtus and the infant,) in which they are capable of living, or the peculiarity of the circulation by which they are enabled to do so. But if we regard the state of the nervous system, the analogy, as far as the brain is concerned, is at once lost. Do not the experiments of Le Gallois, which, contrary to all expectation, established the fact, that, the younger the animal the greater its tenacity of life, suggest the advantage derived from this analogy? Do we not, in the new-born infant, discover the durable irritability of the amphibious, conjoined with the secondary irritability, if I may use the expression, of the more perfect animal? Does not the infant possess the tenacity of life which characterizes the amphibious animal, conjoined with the secondary source of power which belongs more particularly to the mammalia? Is not the life of the infant thus doubly preserved at a time when the lungs perform their action imperfectly; when the primary source of power is limited, and the actions of the system soon followed by exhaustion?

We may, in illustration of our subject,

advance one step beyond the human species. Nature seems, in the peculiar structure of *birds* to have made extraordinary provision for the oxygenation of the system. We find the lungs communicating with all parts of the body, the heart endowed with a supernumerary muscle for the purpose of impelling the blood more forcibly into the lungs, and a nervous system extensively developed. "The respiratory organs of birds constitute one of the most singular structures in the animal economy, on account of several peculiarities which they possess; but more particularly in consequence of their connection with the numerous air cells which are expanded over the whole body."* "All birds possess a very remarkable peculiarity in the structure of the heart. The *right* ventricle, instead of having a membranous valve, such as are found in both ventricles of the mammalia and also in the *left* of birds, is provided with a strong, tense, and nearly triangular muscle. This singular structure assists in driving the blood with greater force from the right side of the heart into the lungs."† It has generally been supposed, that man possessed a larger brain, in proportion to his body, than any of the inferior animals; but we find a striking exception to this rule in birds. According to Professor Soemmering, the brain forms, in man, from $\frac{1}{22}$ to $\frac{1}{33}$ of the body; in the sparrow, $\frac{1}{28}$; the chaffinch, $\frac{1}{27}$; the cock, $\frac{1}{25}$; and in the canary bird, $\frac{1}{14}$. Now it cannot be said that the brain is thus extensively developed in birds, for the purpose of superior intelligence, or superior sensation; it must be proportioned to the exigencies of the body, and not of the mind. If we find that the size of the nervous system is in the inverse ratio of the strength of the animal, being the *largest* in the *weakest*, is it not reasonable to conclude, that the secondary source of power is thus more fully developed, because the primary source is more readily exhausted? Professor Cuvier says, "generally speaking, small animals have a larger brain in proportion to their body than larger ones."* "It will appear that, all things considered, the smaller animals have the brain proportionally the largest; that man is surpassed in this respect only by a small number of animals, all of which are *lean* and *meagre:* as mice, small birds," &c.† Why are they lean and meagre? It cannot be for the want of food, but in consequence of its rapid expenditure. "Digestion, also, has its connection with respiration; the latter being one of the functions which consume and expel, with the greatest rapidity, the substances of which the body is composed; the digestive power is generally the greater in proportion as respiration is more complete, in order that the quantity which is acquired may be equal to that which escapes. This is the reason why birds have, in general, an exceedingly strong stomach, and a very quick digestion."* I have already attempted to shew, that the expenditure, both of food and of air, is proportioned to the *actions* of the animal. This expenditure being great in birds, is a proof of their actions, as compared with the strength of their bodies, being great also; a secondary source of power, therefore, in the nervous system, is particularly required. Thus the brain of the canary bird constitutes $\frac{1}{14}$, whilst that of the eagle is only $\frac{1}{200}$ of the whole body.

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* Encyclopedia. † Ibidem.

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* Comparative Anatomy. † Ibidem.

I shall adduce one more example in favour of the doctrine which I am endeavouring to establish. The brain of the mole is very little inferior, in proportion, to that of man; the latter constituting $\frac{1}{35}$ and the former $\frac{1}{36}$ of the whole body. Can any other reason be given for this peculiarity, this enormous size of the brain, compared with that of other quadrupeds, except that which I have suggested, viz. that the expenditure of oxygen often exceeds the supply? Admitting that warm-blooded burrowing animals are not cut off from *all* communication with the atmosphere, it is clear, I conceive, that the communication is not so immediate, and that in the very act of burrowing, that communication must often be interrupted. The brain of the field mouse is larger than that of man, constituting, in the former $\frac{1}{31}$, in the latter $\frac{1}{33}$ of the whole body; and what is there in the natural history of this animal which may account for this peculiarity? "The harvest mouse is considered as the smallest of British quadrupeds, weighing only the sixth part of an ounce. Its nest is most artificially constructed and platted of the blades of wheat, and is of the size of a cricket ball; the opening to it being *closed up* so skilfully as to be almost imperceptible. Such is its compactness, that it may be rolled over the table without derangement. One found of this description contained eight young, and appeared completely full without the dam, whose mode of access to it, *so close and compact* as it appeared on every side, seemed not easy to be explained. In the winter these animals burrow deep in the earth."* Why should the brain of the rabbit be larger than that of the hare, constituting in the former $\frac{1}{140}$, in the latter $\frac{1}{228}$ of the whole body ? Why should the brain of the dolphin and of the porpoise (which are admitted to be the most active inhabitants of the ocean, and who breathe by lungs and not by gills, and therefore must require frequent communication with the atmosphere) be so particularly developed, constituting in the former $\frac{1}{25}$, and in the latter $\frac{1}{93}$ of the whole body ?" The seal has, proportionally to its nerves, a larger brain than the house dog, and the porpoise more than the orang-outang; yet we do not observe the same proportions in the *faculties* of these animals."† Why should the brain be larger in children than in adults? "Haller remarked that children have a larger brain than adults, in proportion to their body, and consequently that if their faculties were measureable by the proportionate size of the brain, they ought, in understanding, to excel grown up persons."‡ The same author adds, "It may, however, be replied, that the brain of children is not yet perfectly developed." Why should nature have formed a large brain before it is wanted ? Is it for the sole purpose of adding to the difficulty of parturition? Why should this law of nature so generally prevail? "Land insects, which constitute, as it is well known, by far the greatest number of this class of animals, are furnished with air cells or tracheæ, which ramify over most of their body. These tracheæ are much *larger and more numerous* in the *larva state* of such insects as undergo a metamorphosis."* Does not nature here inform us, that the younger the animal the greater the necessity for oxygen? If the nervous system be a secondary source of this principle, we see why, in the younger animals, it should be large in proportion to the size of the body. May it not be assumed, as a general law of nature, that the nervous system is developed, in proportion as from the habits, age, and constitution of the animal, the expenditure of oxygen more frequently exceeds the supply?

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* Comparative Anatomy.

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* Encyclopedia. † Spurzheim. ‡ Ibidem.

The irritability of an organ is not solely dependent upon its oxygen, but is influenced by the structure of the organ itself. Let us take the extremes of the prize fighter and the nervous, sedentary, debilitated hypochondriac. We say that the former, when well trained, is in good wind. What do we mean by the expression? It is simply that the system, by a certain mode of training, has acquired that firmness of texture which enables it to make greater exertions than natural with less expenditure of power. The expenditure of oxygen is slower, and therefore its supply through the medium of the respiratory organs, is not so frequent and necessary as in other individuals. A prize fighter, in this respect, approaches to the amphibious class, and may be denominated a cold-blooded animal in more senses than one. The system, under these circumstances, must be less dependent upon a secondary source of power, and consequently less dependent upon the brain. The nervous hypochondriac, on the contrary, is living upon the brain; he cannot support the ordinary expenditure of oxygen without drawing upon his principal; the muscles are flabby in their texture, and resemble those of infants; the irritability is more exhaustible, the expenditure of oxygen more rapid, and, consequently, the supply through the medium of the lungs more frequent; he cannot run a hundred yards without panting for existence. The parallel between the prize fighter and the diver is, in this respect, remarkable; they both acquire the power of living upon less oxygen. It has been said, that divers have become so expert in the art they possess, as to remain under the water for twenty minutes. Have they acquired the power of amphibious animals, which enables them to live for a certain time upon the nervous influence without the aid of respiration? May not the phenomena of fever, and of what are called nervous affections, depend upon the secondary source of power being depressed or exhausted in the former, and accumulated in the latter? May we not thus distinguish between the fever of health (if I may use the expression) and that of disease, and ascertain the precise point at which the latter commences.*

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* Encyclopedia.

If, then, to an organization possessing life, the *necessary proportions* of moisture, heat, and of the arterial or nervous influence be added, that organization acquires the power of being excited into action; and this is equally the case, whatever be the structure of the organ in question. The degree of irritability will vary according to the nature of the organization, and in some structures, such as bone, tendon, ligament, hair, cuticle, nails, &c. may be altogether deficient, because the necessary proportions of heat, moisture, and oxygen are not present; but these several organs may acquire irritability by a change of circumstances. The term "irritability," therefore, is not peculiar to muscular fibre, but applies to every individual organ, and may be denominated, generally, susceptibility of action. This exists, to a certain extent, in dead animal matter, and is then denominated elasticity, or that power by which the particles of matter, when forcibly separated from each other, are enabled, that force being removed, again to approximate. This property of matter, when appertaining to a *living* organization, constitutes irritability. But as this organization is subject to variations, more particularly as connected with the variable supply and assimilation of food and of air, the elasticity of matter is different from its irritability, inasmuch as the former is the property of matter unendowed with life, and subject to little variation, whilst the living organization is undergoing a constant change, and a consequent variation in that property, which is the effect of heat, moisture, oxygen, *and organization.*

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* By the expression "fever of health," I mean a concurrence of those symptoms which characterize fever, but which are the transient effects of that exertion which is natural, which has been provided for, and which, therefore, cannot be regarded as morbid. The fever of disease does not commence until the secondary source of power is reduced below that standard which is consistent with the usual and ordinary demands of the system.

If, to an organization thus endowed with irritability, a stimulus be applied, action is the consequence. How does a stimulus produce action? What do we mean by the word action? If to an organization, endowed with susceptibility of action, any cause, capable of changing the relative situation of the organic particles, be applied, that cause is the stimulus to the action by which this change is counteracted; and this stimulus may act either mechanically or chemically. It is of no consequence whether this change be produced by the impulse of blood, alimentary matter, chyle, lymph, light, air, sapid or odorous particles, or the resistance of solid bodies; however various the agent which may be peculiar to different organs, the *immediate* stimulus of every organ is precisely the same, viz. a change in the relative situation of the organic particles. I am not aware of any exception to this rule in the animal economy. What then, it may be asked, is the stimulus to voluntary motion, if the nervous influence contribute only to the *susceptibility* of action? Is not every voluntary muscle naturally in a state of tension? Will not every voluntary muscle, when separated from either of its attachments, contract upon itself? Is it not clear, that the stimulus to action is always present in the muscle? How then is voluntary motion produced? By an act of the will the nervous influence is accumulated in muscular fibre, by which its irritability is increased. If the susceptibility of action be thus increased, the stimulus, which was before inefficient, will be sufficient to excite that action which is the consequence, not of any increased stimulus, but of increased susceptibility of action. The living action itself is that counteracting change in the relative situation of the organic particles, by which they contract upon themselves in opposition to that stimulus which has a tendency to separate them. The power of an organ is that property upon which the contraction depends, and will correspond with the degree of irritability, and the nature of the organization. I am not disposed to enter into any farther discussion of this subject, as it does not seem to involve any point of practical importance, and is evidently more speculative than useful.

It has been usual with physiologists to confine their ideas of living motion to muscular fibre, and yet the same physiologists, with marked inconsistency, admit that motion is, where muscular fibre is not. Thus zoophytes are said to have "no muscles nor muscular fibres, no articulated members," and yet their "contractions are frequent, rapid, reiterated; their irritability quick and susceptible of being excited by the stimulus of light only."* "They also admit that the existence of fibres does not imply susceptibility of action. The muscles of the more perfect animals are all fibrous, and therefore the anatomists frequently infer muscularity in parts from the presence of fibres. But they are by no means a certain mark of muscularity. The tendons, aponeuroses, bones, cellular membrane, brain, and nerves, are all of them fibrous, but not at all muscular."† If motion exist where fibres do not, and fibres exist where motion does not, with what consistency can we confine our ideas of motion to muscular fibre? The most rational view of the subject is suggested by the author last quoted. "On the other hand, as the ultimate or constituent fibres are invisible in the best microscope, parts may be fibrous and muscular, though they appear otherwise to us, or they may have contractile power *without being fibrous*." I conceive, then, that *every species* of living organization, which is capable of having the relative situation of its organic particles altered by a natural and appropriate stimulus, is capable of counteracting that change to a certain extent, which implies motion; and thus the principle of elasticity, contractility, tonicity, and muscularity, is precisely the same. Why, then, should we deny to veins, lymphatics, and nervous fibre, and indeed to any structure possessing irritability, the power of being excited into action? All living action, then, is motion.

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* Dumas. † Cruickshank.

We have already seen that heat, moisture, and oxygen (in the form either of the arterial or nervous influence) are essential to the existence of living action. Is the action of muscular fibre attended with an expenditure of its moisture and oxygen? The question may surely be answered in the affirmative, because we find that the effect of increased action is to render necessary an increased supply of liquid ingesta and of oxygen. The coal-heaver, without a due supply of liquids, would soon dwindle into "a substance without a shadow in the sun." Dr. Paris says, "It has been *satisfactorily established*, by physiological researches, that the demand for oxygen in an animal body, will be in proportion to its expenditure by muscular exertion."*

The loss of the more ponderable parts of the system, as the effect of action, may be inferred from the increased supply of food which is found to be necessary, and is proved by actual experiment.

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* Med. Jurisprudence.

The phenomena of living action, then, are dependent upon the same principles in every part and structure of the body. There must be an organization compatible with life and action—a certain degree of heat, moisture, and oxygenation; a stimulus, that is, a change in the relative situation of the particles; and a power of resisting that change. When these circumstances concur, motion will always take place. The effect of motion, in every part of the body, is the expenditure of the organic particles, which had, by the process of assimilation, been elaborated from the food and air into component parts of the system.

CHAPTER XV.

Digestive Functions.

RESPIRATION.

In the term "Digestive Functions" I include all those living actions, by which the food we take and the air we inspire, are changed, modified, incorporated with the system, and ultimately eliminated from it. As these actions are more immediately and more ostensibly connected with the supply of air than of food, I shall reverse the arrangement before adopted, and commence with the consideration of the former. Respiration has been said to consist in drawing a certain portion of air into the lungs, and throwing *it* out again. The same idea is conveyed by some writers, who, having considered the subject more minutely, cannot be supposed to have disregarded the agency of the residual air, which is always present in the lungs. Thus Dr. Henry says, "the function of respiration consists of two distinct actions, that of inspiration, by which the air is drawn into the lungs, and that of expiration by which *it* is expelled, after having served the purpose for which it was intended."* As the inspired air must unite with that already contained in the lungs, the expired air will necessarily be a compound of both. Respiration may be more accurately defined as that process by which a certain portion of air, containing 79 per cent. of nitrogen, and 21 of oxygen, is taken into the lungs; and by which a corresponding portion of air, containing 79 per cent. of nitrogen, 13 of oxygen, and 8 of carbonic acid, is expelled.

Various opinions have been entertained as to the quantity of air taken into the lungs by an adult in each inspiration. Dr. Henry says, "By an easy, natural inspiration, about 20 cubic inches of air may, perhaps, on an average, be the quantity taken in."† "As there seems to be no obvious source of inaccuracy in the processes employed, and their results so remarkably coincide, and as they present the average bulk deduced from 56 respirations, we may conclude, says Dr. Bostock, that 40 cubic inches of air are the quantity employed in an ordinary act of respiration."‡ As the quantity of air inspired is nearly in the ratio of the time employed, and as, in the experiments of Messrs. Allen and Pepys, about 3900 inches were respired in eleven minutes, I am disposed to adopt the calculation of Dr. Henry, and to rate the average quantity of each inspiration at about 20 cubic inches. "Thus (says Dr. Menzies) if the bulk of air commonly inspired be estimated at 40 cubic inches, and the number of respirations at 18 in a minute, the volume of air inspired every minute will be 720 cubic inches (that is, 7920 in eleven minutes)."* Allowing 20 cubic inches to each inspiration, the quantity respired in eleven minutes would be 3960 cubic inches, which very nearly corresponds with the experiments mentioned. The quantity expired is found, by experiment, to be nearly the same as that taken in.

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* Chemistry. † Ibidem. ‡ Ellis's Inquiry.

Various opinions are entertained as to the quantity of residual air remaining in the lungs after a natural expiration. "By a very different mode of experiment, we find Mr. Davy to conclude that his lungs, after a *forced* expiration, contain only 32 cubic inches, when it is reduced to the temperature of 55, but which, by the heat of the lungs and saturation with moisture, are increased to 41 cubic inches; and after a *natural* expiration they contained

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* Ellis's Inquiry.

118 (say for the sake of argument 120) cubic inches."*

The lungs, then, of an adult, in a natural state of expansion, may be calculated to contain 140 cubic inches of air, and 20 cubic inches are inspired and expired 18 times every minute.

"Atmospheric air, after being once only admitted into the lungs, returns charged with 8 or 8½ per cent. of carbonic acid. If the same portion be breathed repeatedly, considerable uneasiness is experienced, but the quantity of carbonic acid cannot be increased beyond 10 per cent."† Consistently with this view of the subject, Messrs. Allen and Pepys have ascertained, that 100 parts of atmospheric air consist, of nitrogen 79, oxygen 21; and 100 parts of expired air consist of nitrogen 79, oxygen 13, carbonic acid 8. Now if we suppose the 20 cubic inches of air inspired to lose 8 per cent. of oxygen, and acquire 8 per cent. of carbonic acid in every respiration, the idea is clearly erroneous, because the 20 cubic inches of air containing no carbonic acid, unite with 120 cubic inches, containing at least 8 per cent. If, after a natural expiration, we continue, instead of again inspiring atmospheric air, to expel by forced expirations the residual air from the lungs, that residual air will be found to contain at least 8 per cent. of carbonic acid. If, therefore, *without any change*, that is, after a simple union of the inspired with the residual air, 20 cubic inches were expired (the difference between 120 and 140 being only $\frac{1}{7}$), the expired air would contain 9.6 of carbonic acid in 140 instead of 120, which is only a fraction less than 7 per cent. How does this apply to the oxygen? Expired air contains 13 per cent. of oxygen. Suppose 20 cubic inches of atmospheric air (that is, nitrogen 15.8, oxygen 4.2) to be inspired and added to 120 cubic inches of residual air (that is, nitrogen, 94.8, oxygen 15.6, carbonic acid 9.6,) what would be the proportions? Nitrogen 110.6, oxygen 19.8, carbonic acid 9.6, which is rather more than 14 per cent. of oxygen; so that by the simple union of the respired and residual airs, the oxygen is diminished 7 per cent. and the quantity of oxygen lost in each respiration is 1, not 8 per cent. The change, then, which takes place during every respiration, is the loss of 1 per cent. of oxygen, and the acquisition of 1 per cent. of carbonic acid. "When the state of the expired air is examined by eudiometrical tests, a quantity of oxygen is found to have disappeared, equal in volume, according to the experiments of the same accurate chemists, to the carbonic acid which has been formed. The only change, then, which has been satisfactorily proved to take place, is the removal of a certain quantity of oxygen (its nitrogen being wholly untouched), and the substitution of a precisely equal volume of carbonic acid."*

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* Ellis's Inquiry. † Henry's Chemistry.

Much importance has been attached to this correspondence between the oxygen lost and the carbonic acid found ; and it has been supposed, that the whole of the oxygen lost is contained in the carbonic acid generated ; and yet, from whatever part of the system arterial blood be drawn, it presents those characters which we attribute to the presence and agency of oxygen ; and we do so, because we find that oxygen imparts to blood out of the body, those properties which distinguish arterial from venous blood. Now the oxygen lost cannot exist in the carbonic acid expired *and* circulate through the system. How are these contradictions to be reconciled? It must be admitted, that the residual air in the lungs corresponds with that which was last expired, that is, contains nitrogen 79 per cent., oxygen 13, carbonic acid 8. If 20 cubic inches of inspired air, (that is, nitrogen 15.8, oxygen 4.2,) be added to 120 inches of residual air, (that is, nitrogen 94.8, oxygen 15.6, carbonic acid 9.6,) what would be the proportions, supposing no change but a simple union of the inspired and residual airs to take place? Nitrogen 110.6, oxygen 19.8, carbonic acid 9.6, that is, 14.2 per cent. of oxygen, and 6.8 of carbonic acid; deduct from the oxygen and add to the carbonic acid 1.2, and the proportions of each will be 13 and 8 per cent. which exactly corresponds with the expired air. So that the conversion of 1.2 cubic inches of oxygen into the same quantity of carbonic acid seems to take place in every respiration. But if we pursue the subject farther, we shall find that this proportion between the oxygen lost and the carbonic acid generated, is not maintained. Sir H. Davy found that hydrogen gas could be respired with impunity, and that, in its passage through the lungs, it experienced no change. Here, then, was an easy mode of respiring the residual instead of atmospheric air; and what was the result? "The bulk of this residual air reduced to the temperature of 55, he estimates at 32 cubic inches, being composed of nitrogen 23, oxygen 4.9, carbonic acid 4.1, which is 15.3 per cent. of oxygen, and 12.8 per cent. of carbonic acid. Now, in proportion as the carbonic acid increased from 8 to 12.8 per cent. the oxygen should have diminished from 13 to 8.2 ; but the carbonic acid, has increased 4.8 per cent. and the oxygen, instead of being diminished 4.8 per cent. is increased 2.3, so that there is a surplus of 5 per cent. of oxygen. The respiration of plants is found to produce the same changes in the atmosphere, that is, the same loss of oxygen and substitution of carbonic acid as that of animals. If, therefore, carbonic acid be generated by plants, without any communication with the atmosphere, it is clear that the oxygen contained in the carbonic acid must be derived from some other source than the atmosphere, that is, from the plants themselves. "Mr. Cruickshank found, that when carbonic acid was produced by steeped seeds, confined either *in nitrogen or hydrogen gas*, the bulk of air was increased one-fifth, but nothing like germination then took place. He even found that seeds, after being soaked in water, and passed up into a tube of *mercury*, formed carbonic acid in large quantity, but without undergoing any sensible change in their appearance. This experiment was repeated by passing up half a dozen soaked peas into a graduated tube *filled with mercury*, and immersed in a jar of the same fluid. By the following day two-tenths of a cubic inch of air were collected in the upper part of the tube, and the experiment was continued till the bulk of air was increased to 1.6 cubic inch, beyond which it did not proceed. Lime water was then passed into the tube, and with the aid of a little agitation, all the air except a small bubble, was rapidly attracted by it; the peas did not exhibit the slightest sign of germination."* The author supposes that the carbonic acid is generated, not by any living action of the seed, but by the decomposition of its structure. It is clear that, in these experiments, the oxygen was derived from the seed itself; but I do not think it equally clear, that the carbonic acid was not generated by a living action of the seed. There was no appearance of germination, but Mr. Huber found, "that a quantity of carbonic acid was produced when seeds were placed in a jar of *nitrogen* gas, and duly supplied with water; and that, in a few days, *the radicle, in a small degree, was protruded,* which he considers as a proof of germination."† This apparent growth is attributed by Mr. Ellis "to the swelling of all the parts of the seed from the imbibition of water," which is clearly an imaginary explanation, and may be admitted, or not, at the option of the reader. The oxygen, I may add, could not be obtained from the water itself without decomposition, and there is no evidence of such decomposition having taken place. We have every reason to suppose that it does not take place, because no hydrogen is evolved. The oxygen, then, contained in the carbonic acid, must be derived from the seed itself. Admitting with Mr. Ellis, that the carbonic acid was the product of vegetable decomposition, is not the process of decomposition constantly going on in the animal frame? Why may not the oxygen contained in the carbonic acid expired, be derived from that source? If, then, the proportions between the oxygen lost and the carbonic acid generated, are not always maintained, is it not probable, that the occasional correspondence is dependent upon some other cause, and that, in point of fact, the oxygen contained in the carbonic acid expired is derived from some other source than the air in the lungs? We have already seen that a constant supply of oxygen is necessary to the support of living action, and this implies that a constant expenditure is going on. The actions of the system, then, are sources of oxygen. Supposing the carbonic acid to be excreted by the pulmonary arteries, may not the action itself be attended with an evolution of oxygen equal to the carbonic acid generated? May not the carbonic acid expired be wholly an excretion, and the oxygen which it contains be that, which, having been assimilated with the system and contributed to the support of living action, has become excrementitious? If so, the whole of the oxygen lost may unite with the blood, circulate through the system, and become assimilated with it; and the oxygen contained in the carbonic acid expired may be that, which, having united with the excrementitious fluids of the body, having, (through the medium of the lymphatic absorbents,) been united with the blood, and having, probably, entered into some new combination, is, by an excreting action of the pulmonary arteries, again separated from the blood, and contained in the carbonic acid generated.

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* Henry's Chemistry.

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* Ellis's Inquiry. † Ibidem.

The correspondence between the oxygen lost and the carbonic acid generated, that is, in air which has been once only respired, admits of some explanation. Instead of commencing our view of respiration with the air inspired, let us go one step farther back. There are always present in the lungs, air capable of imparting oxygen, and blood in a fit state to receive it. What, then, is going on in that period which intervenes between each inspiration? The blood is *constantly* taking oxygen from the air. The air contained in the lungs is losing oxygen. When this oxygen is reduced to a certain point below 13 per cent. another inspiration becomes necessary; and that this inspiration is the consequence of, and is proportioned to, the de-oxygenation of the air contained in the lungs, we may infer from the following experiment:—If immediately after inspiration, that is, when the lungs contain on an average about 140 cubic inches of air, the breathing be checked as long as the necessities of the individual will admit of, the next expiration is not a natural expulsion of 20 cubic inches of air, but a forced expulsion of 40, 60, or 80 cubic inches, proportioned to the period during which respiration has been checked. Now the *quantity* of air in the lungs is but little altered; the forced expiration, therefore, can have no connection with the quantity, but with the quality of the air contained in the lungs. If, then, in five seconds, the oxygen of the air be reduced 1.2 cubic inches, in 10 seconds 2.4, in 20 seconds 4.8, and in 40 seconds 9.6, the quantity of air expired will be such, as to enable the next inspiration of atmospheric air to raise the residual air contained in the lungs to its proper degree of oxygenation. If, then, in the period which intervenes between each inspiration, the blood take 1.2 per cent. of oxygen, 20 cubic inches of air will be expired, because the same quantity of atmospheric air inspired, and simply united with the residual air contained in the lungs, will restore the 1.2 per cent. of oxygen which has been lost; so that the quantity inspired has no relation to the carbonic acid given out in the following expiration, but to the state of the residual air in the lungs previous to inspiration. As the periods which intervene between each natural inspiration are nearly synchronous, the loss and supply of oxygen in each respiration will be nearly the same. As, in health, the quantity of carbonic acid excreted will be, cæteris paribus, in the ratio of the time employed, it follows that the same quantity will be given out at each expiration, and therefore, whether we compare the quantity of oxygen lost with the carbonic acid given out in the previous or subsequent expiration, the proportions will be the same. Why, then, it may be asked, do the pulmonary arteries excrete the same quantity of oxygen as the blood takes from the air in the lungs? I have already shewn that the fact does not apply to air which is frequently respired, and the fact, as applied to air once only respired, is far from being established. Mr. Ellis says, "As this opinion rests on the belief, that the quantity of oxygen gas that disappears in respiration, is not sufficient to account for the bulk of carbonic acid produced, it can no longer be maintained, if it be shewn that the volume of that gas which is lost, *exceeds* that of the carbonic acid found. Now this has been *amply done* in the experiments already given, and, indeed the *excess* of oxygen lost was so apparent and so constant, as to lead Lavoisier and others after him, to conclude that it was employed to form a part of the water expelled from the lungs, by uniting with hydrogen supposed to reside in the blood."* I have already shewn that carbonic acid is generated by seeds without any supply of oxygen; and that in the experiment of Mr. Huber, it appeared to be the product of a living action. Mr. Ellis regards the protrusion of the radicle, under these circumstances, as no proof of living action, but as the mechanical effect of moisture; and yet, in a former part of his work, he says, "Although water and heat appear to be the only agents essential to the *beginning*

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* Ellis's Inquiry.

of germination, yet, *after a certain period*, air becomes equally necessary." Might not germination begin without being apparent, and thus reconcile the two passages which appear to be in opposition to each other? The explanation of the fact appears to be this:—The seed already possesses oxygen in itself, which it had previously derived from the atmosphere; but the presence of oxygen alone does not imply that of irritability, because irritability is the property of a peculiar organization, of which oxygen must form a part, but to which a certain degree of moisture must also be added. The seed, then, when moistened, begins to germinate without any external supply of oxygen, because the system already possesses as much oxygen in itself as is sufficient for the commencement of germination; and this oxygen, excreted in union with the carbon of the seed, forms the carbonic acid in question. There must be a law in the system by which the supply of oxygen corresponds with the expenditure, otherwise the equilibrium of health could never be maintained. How does this law operate in the lungs? The pulmonary arteries excrete, during every respiration, 1.2. per cent. of carbonic acid, and the blood takes from the air a corresponding portion of oxygen; that is, the quantity of oxygen absorbed bears some relation to the quantity of oxygen excreted. Twenty cubic inches of air are then expired, because that quantity of atmospheric air is necessary to restore the residual air to its natural degree of oxygenation. Supposing 2.4 per cent. of carbonic acid to be excreted, and a corresponding portion of oxygen to be absorbed, the next expiration will be, for the reasons given, 40 cubic inches; and thus the correspondence in question, is not between the oxygen lost and the carbonic acid subsequently generated, but between the carbonic acid previously generated, and the state of the residual air which regulates the quantity of air expired. I have already shewn that the proportions are far from being accurately preserved under all circumstances; but it seems to be perfectly consistent with reason, that some relation should exist between the oxygen excreted and absorbed by the lungs. The lungs it may be said, are not the only organs by which oxygen is excreted from the system, and the expenditure, according to this view of the subject, would greatly exceed the supply. But it must be remembered, that the lungs are not the only organs by which oxygen is supplied to the system. The ingesta taken into the stomach are manifest sources of oxygen.

Whence is the carbon derived? Evidently from the system itself, because there is no other source of supply. Is it derived from our food, or rather from the chyle after its assimilation with the blood? Carbon constitutes a component part of the animal frame, and is derived from the chyle. To suppose that *this* carbon, which is capable of incorporating with the system, and of thus contributing to the support of its organization and action, is excreted *before* it has answered these important purposes, is to imagine a very unnecessary expenditure of an ingredient, in the formation of which the most important organs have been employed, and which is essential to the organization of the system. We have already seen that the action of an organ is productive of a change in its elementary particles, by which they lose their vitality, become excrementitious, and are, through the medium of the lymphatic absorbents, reunited with the blood, for the purpose of being eliminated from it. Is there not an evident source of excrementitious carbon; and is not the carbonic acid generated wholly an excretion, deriving both its oxygen and its carbon from the *débris* of the system? If the carbon, which has been assimilated with the system and thus acquired vitality, be deprived of that vitality and become excrementitious—(and that such a change is going on may be inferred from the constant supply which is found to be necessary, the correspondence between the action of the system and that supply, and the experiments by which madder was found to give a peculiar colour to the bones, which colour again disappeared)—if, I say, the carbon become excrementitious, there must be an organ or organs by which it is eliminated from the system; and if so, why not by the lungs?

How, in the process of respiration, does the oxygen unite with the blood? Mr. Ellis conceives that the organization, the anatomical structure of the air cells and blood vessels, are incompatible with the idea that such an union ever takes place; that the thing is impossible. The experiments of Dr. Priestley are, in my opinion, sufficient to establish the fact, that oxygen is capable of uniting with blood, although animal membrane intervene between the two. Is not the fact admitted by Mr. Ellis himself, when he quotes the authority of Bichât in support of the opinion, that air uniting with the blood is productive of great general distress to the animal? "Air (says M. Bichât) thrown into the vascular system, quickly brings on agitation, convulsions and death. By forcing air through the windpipe *into the lungs* with a syringe, *and confining it there*, he has made it *to enter into the blood vessels*, which immediately brings on agitation and exertion in the animal; and if an artery in the leg or foot be now opened, the blood will spring out frothy *and full of bubbles of air*. If hydrogen gas has been used, *the bubbles may be inflamed*."* To say, in opposition to this fact, that air *cannot* penetrate blood vessels, which, according to the same author, " become, at length, inconceivably minute, forming a plexus or *fine* network upon the proper cells," is assuming what it would be extremely difficult to prove, and what cannot, without proof, be maintained against the apparent testimony of our senses in the experiments of Dr. Priestly, and the absolute proof in those of M. Bichât. Thus if oxygen, uniting with the blood out of the system, impart to venous the scarlet colour of arterial blood, and if, when atmospheric air is introduced into the lungs, venous blood has been *seen* to become scarlet, and upon the supply of air being interrupted, to become dark and continue so until the air is again supplied; shall we deny such evidence; shall we deny the fact merely because we cannot account for it? I conceive, then, that the whole of the oxygen lost unites with the blood and circulates through the system.

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* Ellis's Inquiry.

Various opinions have been entertained as to the mode of this union—as to the action by which the effect is produced:—some supposing that the oxygen lost penetrates the blood vessels, unites with the carbon, and is *immediately* thrown off in the form of carbonic acid; others, that it unites with the blood, and, in the course of circulation, with the carbon, in those proportions which constitute an oxide of carbon, and, upon its return to the lungs, unites with an additional quantity of oxygen, by which it becomes carbonic acid, and is then excreted; and others, that the *atmospheric air* unites with the blood, and is decomposed in the blood vessels of the lungs, and that the oxygen unites with the carbon of the blood, in those proportions which constitute carbonic acid. Now I differ with these several opinions, inasmuch as I do not conceive that the oxygen taken into the blood unites with the carbon at all, but circulates through the system for the purpose of being incorporated with it. It has been a question whether the air be decomposed by the living action of the lungs, or by the chemical action of the blood. I am inclined to adopt the latter opinion, because that change in the colour of the blood, which we attribute to the agency of oxygen, takes place externally to the body when blood is exposed to the influence of the atmosphere. If the living action of the lungs be at all concerned in the change we witness, it is reasonable to suppose that a division of the eighth pair of nerves, by impairing that action, would influence the change in question. M. Dupuytren asserts that the oxygenation of the blood will not take place under these circumstances. "Le resultat de ses recherches fut que les animaux auxquels on les a coupés, meurent constamment d'asphyxie. Il en trouva le preuve non seulement dans la dyspnée qui a constamment lieu, mais encore *dans la couleur* du sang arteriel qui devient *de plus en plus noire* comme dans l'asphyxie."* The experiments of M. Dumas were attended with directly opposite results. "Il trouva qu'en soufflant de l'air dans les poumons de ces animaux après leur avoir coupé la paire vague, il se forme du sang arteriel, lequel à une *aussi belle couleur vermeille* qu'auparavant."† The experiments of

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* Le Gallois. † Ibidem.

M. Le Gallois correspond with those of the latter author, and lead him to conclude, that the oxygenation of the blood is not dependent upon any action of the lungs themselves. He conceives that, in the experiments of M. Dupuytren, the blood remained dark and venous, in consequence of the air-passages being so obstructed that no oxygen was supplied. If, therefore, the oxygenation of the blood be not interrupted by impairing the action of the lungs, provided the air itself be artificially supplied, we may conclude that the atmospheric air is decomposed by the chemical action of the blood. It is not easy to say what part of the blood presents a stronger attraction for oxygen than the nitrogen with which it is already united ; but I am disposed to think, that the serum may be the principal agent in decomposing the atmosphere ; because the serum alone, as may be proved by experiment, is capable of producing those changes in the air which are the known and acknowledged effects of respiration ; and because I have frequently observed, that if the crassamentum of blood, drawn from the system and allowed to coagulate in a vessel, be raised above the surface of the serum, it will become dark, whilst that which is not so immediately exposed to the atmosphere, and remains covered with the serum, will become and continue florid. I have also found, that if the serum be constantly poured off from the surface of the crassamentum, the latter does not acquire and retain the florid colour of oxygenation. The oxygen of the air, then, containing, like every other substance, electricity (which is, probably, nothing more than caloric in a certain state of modification) unites with some part of the blood, perhaps the serum, and in consequence of this new combination, or of its natural quantity of electricity being altered, loses its characteristic chemical properties, and in this state constitutes the arterial influence. The elements of the air are disunited; the oxygen, its nutritious part, enters into a new combination, by which a new product is formed, essentially different from that element itself; the nitrogen, the excrementitious portion of the atmosphere, is, as soon as convenient, eliminated from the system, and thus the process of respiration is a process of digestion.

Respiration is a compound action in which we observe the combined influence of mechanical and chemical agents. The cavity of the chest is expanded and contracted by the mechanical action of certain muscles, and the change in the state of the air is effected by the chemical action of the blood.

The muscles by which this function is supported are, with the exception, probably, of the diaphragm, both voluntary and involuntary. The involuntary action of these muscles, subservient at other times to the will, may be thus explained :—The muscles of respiration are, from the moment of birth, in constant action; there must, therefore, be a constant supply and expenditure of that influence upon which their irritability or power of action is dependent. They are also in a constant state of tension, and therefore the stimulus to action is always present. The irritability is diminished by every contraction, because it is attended with an expenditure of that principle upon which it is dependent. As the expenditure is constant, so also must be the supply. The irritability being thus restored to that point which renders the stimulus efficient, contraction will take place involuntarily. The irritability may, at any time, be raised to this point by the will of the animal, that is, by the power which it possesses of accumulating this principle in certain muscles of the body, and then voluntary action will take place. The correspondence between the state of the residual air and the action of the respiratory muscles may thus be accounted for. The air becomes vitiated according to the time during which its expulsion is suspended, and the irritability of the muscles accumulates in the same proportion. When, therefore, action takes place, its degree of force will be proportioned to the time during which the air has been losing oxygen, and the irritability has been accumulating. It is probable that, in cases of *irregular* action of the respiratory muscles, vitiated air becomes a source of distress to the lungs, and the sympathetic action of the expiratory muscles takes place upon the same principle as cough and sneezing are excited by irritation of the bronchial or schneiderian membrane. Why, it may be asked, do not all voluntary muscles act involuntarily, when their irritability is accumulated to that point which, consistently with this view of muscular action, should render the stimulus to action efficient? Because the muscular power is kept in a state of equilibrium by antagonist muscles: when these are weakened, as in paralysis, a *permanent* contraction of those muscles which are not weakened may be observed; which, by the by, seems to indicate that the stimulus to action is *always* present in muscular fibre. No such antagonist muscles are opposed to those of respiration (although at the will of the animal certain muscles may become indirectly so), and therefore involuntary action takes place. As the constant action of the respiratory muscles implies a constant expenditure and supply of the nervous influence, these muscles will immediately become paralytic upon that supply being interrupted; and, therefore, any injury of that part of the spinal marrow, upon which these muscles, and particularly the diaphragm, are dependent, will, by suspending respiration, almost immediately destroy life.

It has been calculated that all the blood in the body circulates through the lungs every three minutes. Supposing 2oz. of blood to be sent to the lungs by every contraction, and the contractions of a healthy adult to be 70 in a minute, the quantity of blood transmitted to the lungs would be 340oz. or 26lb. 4oz. every three minutes, which is about the average quantity of blood contained in the system. Now we are not to suppose that the individual 2oz. of blood, which are transmitted to the lungs by every contraction of the right ventricle, are received by the left before the next contraction takes place, and that only 2oz. of blood are ever present in the lungs; but the quantity of blood contained in the lungs is proportioned to their capacity; and the contraction of the right ventricle only gives an impulse to that blood which is already contained in the lungs, by which 2oz. are, by the *vis a tergo*, or by the action of the pulmonary vessels themselves, impelled into the left auricle, and received by the corresponding ventricle. The quantity of blood, therefore, always present in the lungs, will be proportioned to the difference of capacity between the lungs and the right ventricle; and in proportion to this difference will be the period during which that blood is receiving oxygen and excreting carbonic acid.

Consistently with this view of the subject, there are always present in the lungs, air capable of imparting oxygen, and blood in a fit state to receive it. The de-oxygenation of the air is *constantly* going on, and when the quantity of oxygen is reduced to a certain point below 13 per cent., as much air is expired as will enable the same quantity of atmospheric air to restore the due equilibrium of oxygen; that is, to raise the air contained in the lungs to its proper state of oxygenation. So that the air inspired does not *suddenly* penetrate the blood vessels, *suddenly* unite with carbon and form carbonic acid, but simply unites with the residual air contained in the lungs. Neither is the carbonic acid suddenly formed by the union of the oxygen inspired with the carbon of the blood, but is gradually excreted by a process wholly unconnected with that oxygen which is immediately derived from the atmosphere; and thus the whole of the oxygen lost unites with the blood, for the purpose of being incorporated with the system.

CHAPTER XVI.

DIGESTIVE FUNCTIONS.

CIRCULATION.

THE blood may be regarded as a central point, in which the food we take, and the air we inspire, are, both in their nutrient and excrementitious states, that is, before and after they have accomplished their destined functions in the system, united. No apparent advantage would accrue from the actions of the circulating system being placed under the command of the animal. If, indeed, the will of the animal were the primary cause of circulation, the first sleep would be death. The action of the heart and arteries, therefore, is wholly independent of the will, but is not, as some have imagined, independent of the nervous system. This fact I consider to be fully established by the acknowledged sympathy between affections of the mind or injuries of the brain, and the state of the circulation; and might be inferred from the circumstance, that arteries are every where accompanied by nerves. And yet, in plants, the circulation is carried on without nerves, and in zoophytes, in which no nervous system has been discovered; and we learn by experiment and observation, that the action of the heart and arteries may be supported for a certain time, after that of the nervous system has been suspended by disease, artificially or naturally produced.

I have already attempted to shew, that the action of the heart could not, under all circumstances, be supported by the arterial influence alone, and that the power of the heart could never be maintained or restored by the *stimulus* of the nervous influence. As the action of muscular fibre may be suspended or restored, by interrupting or supplying either the nervous or arterial influence, and as this action appears to be occasionally supported by the agency of either, it is reasonable to suppose, that the effect produced by each being precisely the same, the cause is also the same. The action of the heart and arteries appears to be dependent upon the same principles as that of muscular fibre in general; but the heart, in consequence of its proximity to the lungs, the primary source of power, appears to be less dependent upon the nervous than the arterial influence; the arteries, on the contrary, and particularly their secreting extremities, are, in a peculiar manner, dependent upon the nervous influence. Thus, in the experiments of Mr. Brodie, when the action of the heart was supported by artificial respiration alone, not a single drop of urine was secreted. Why? Because the influence of the nervous system, without which the extremities of the arterial system are incapable of acting, had been interrupted by the removal of the brain. It is, I conceive, an important physiological fact to establish, that organs become dependent upon their nerves, in proportion to their distance from the heart.

If we admit that the nervous system is a secondary source of power, we see why the fact should be so; we see the advantages which are derived from its being so; and I am disposed to think, that the phenomena of disease will bear me out in the opinion that it is so. I might, in support of this fact, adduce many of the phenomena which characterize nervous, mental, and bilious diseases; but the consideration of these phenomena would lead me into a train of reasoning wholly inconsistent with this part of my subject; and a fact of so much practical consequence must correspond with our *general* view of disease, or be wholly untenable. Consistently with this view of the subject, I coincide with those authors who have regarded the arteries as not merely passive tubes, but as organs endowed with the power of action, and materially contributing, by that action, to the support of the circulation. This, it is well known, was the opinion of Mr. Hunter, and Bichât distinctly says, that he has *seen* the capillary arteries in action, after that of the heart had positively ceased. In a case of fever, I was much struck with the occurrence of the following circumstance:—the pulse, when the *usual* degree of pressure was applied to the radial artery, was 100 in a minute; but, when a *slight* degree of pressure, just sufficient to enable me to feel the artery, was applied, the pulse was measured distinctly at 140. Here, then, appeared to be a distinct action of the heart and arteries. I conceive that the usual degree of pressure was sufficient to repress the action of the artery, and that the pulse then indicated to the finger the action of the heart alone; but that the action of the artery itself was indicated, when the pressure applied was not sufficient to interrupt it. The following observation of Dr. Wilson Philip's, appears to be connected with the same fact. "Those who have been much in the habit of examining the different states of the pulse, must be aware, that its hardness is most perceptible when a slight degree of pressure is employed. A certain degree, by greatly compressing the vessel, will give some feeling of softness to the hardest pulse, and a slight degree of hardness is not perceptible with the pressure generally employed in feeling the pulse. If the pressure be gradually lessened till it comes to nothing, it often happens that a distinct hardness of pulse is felt before the pulse wholly vanishes under the finger, when no hardness can be distinguished in the usual way of feeling it."* Much has been said of the elasticity, tonicity, contractility, and muscularity of the arteries; but as living action appears to be nothing more than the approximation of the particles of matter, whose relative situation had been previously altered by mechanical or chemical agents, I see no difference, except in degree, between the *living elasticity*,† and the muscularity of an artery. It is not easy to explain the phenomena of venous circulation, consistently with the idea that the veins are merely passive tubes, having no contractile power, and not at all contributing to the support of the circulation. If, in the inferior animals, we observe motion, but cannot discover muscular fibre, are we justified in saying, that veins have no contractile power, because no muscular fibre has been discovered? If the venous circulation be supported by the *vis a tergo*, that is, by the impulse of the heart, the circulation of venous blood ought to give *some* indication of the momentum given by that impulse, similar to what is observed in the circulation of arterial blood. Considering the nature of the communication between the extremities of arteries and the origins of veins, I have some difficulty in conceiving how the capillary arteries can give a momentum to venous blood, sufficient to support the circulation. We seem, indeed, to admit that the venous circulation is not supported by the *vis a tergo*, that is, by the impulse of the heart or arteries, because, in explaining the phenomena of venous circulation, we have recourse to the agency of another power—the action of neighbouring muscles. What assistance do the spinal veins derive from this source? How is the circulation supported in these? We speak of the secreting extremities of arteries;—now, to be consistent with ourselves, we must conceive that bile is elaborated by the secreting extremities of the vena portæ.

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* On Indigestion. † Vide "Living Action."

Does not this necessarily imply an *action* of the venous extremities? If the living elasticity of a vein differ, in degree only, from that of an artery, we have no difficulty in accounting for the phenomena of venous circulation and venous secretion. The peculiarities of the circulation in different animals have already been adverted to; but, I may observe, that such peculiarities are generally connected with that part of the circulation upon which the oxygenation of the blood is dependent. It would appear, then, that the supply of oxygen is of more immediate consequence than that of nutriment; and this might be inferred from the circumstance, that a *constant* supply of the former, and an occasional supply only of the latter, are found to be necessary in the great proportion of animals. I might support my opinion of the local and independent action of arteries, and, perhaps, I might add, of veins, by recording a variety of examples, in which the circulation has been supported without a heart. There is a peculiarity in the circulation of tardigrade animals, noticed by Mr. Carlisle, which appears, in a peculiar manner, to point out the connection between the expenditure and the supply of oxygen. Mr. Carlisle observed, that, in slow-moving animals, the arteries distributed to the muscles, instead of being arborescent, were, at their entrance into the upper and lower limbs, suddenly divided into a number of equal-sized cylinders, and upon this singular fact he makes the following observations:—"The effect of this peculiar disposition of the arteries, in the limbs of these slow-moving quadrupeds, will be that of *retarding* the motion of the blood." In other words, the expenditure of oxygen, corresponding with the nature of the action, the blood is de-oxygenized but slowly, and therefore a diminished supply of oxygen is sufficient for the demands of the animal. The author adds, "Certain it is, that a larger proportion of arteries is sent to the muscles of quadrupeds than to the ordinary substances, and the *extreme redness* of these organs shews that their capillaries are of larger diameter (might not the author have added, that the blood is more oxygenized?) *A greater degree* of redness is also observable in those muscles of the same animals which are most frequently called into action."*

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* Philosoph. Transact. 1804.

CHAPTER XVII.

Digestive Functions.

NERVOUS ACTION.

A LARGE portion of that blood, which, having recently passed through the lungs, is accumulated in the left side of the heart, ascends to the brain and spinal marrow arterial, and returns to the right side of the heart venous. What has become of the arterial influence of that property, to the existence of which oxygen is essential? It may be observed that a *portion* only of the blood contained in the left ventricle is sent to the brain and spinal marrow, and that the remainder is distributed through the system. This also returns to the right side of the heart venous. That action, therefore, by which arterial blood becomes venous, is not accomplished by the brain and spinal marrow alone. I have already stated, upon the authority of the best writers, that nerves are in reality brain distributed through the system. Is arterial blood rendered venous by the action of the brain, spinal marrow, *and* nerves?

It seems necessary to admit the fact, because we find that such a change is produced by the brain and spinal marrow, and because the structure of nerves corresponding with that of the brain and spinal marrow, we must, to be consistent with ourselves, attribute to the nerves a corresponding function. Is arterial blood rendered venous by the nervous system alone? The respiration of plants would seem to show, that the influence of the air is expended by some other action than that of nerves; and M. Bichât says that the irritability of a *muscle* can be alone maintained by duly oxygenized blood. Mr. Brodie has gone so far as to say "That many reptiles which are capable of respiring by means of the skin, will survive the *loss of the brain* for so long a period, that the wound made by decapitation becomes cicatrized, and death only takes place at last in consequence of inanition."* Is it not clear that, in these cases, something tantamount to the influence of the brain is conveyed through the medium of the skin? If life can be supported by cutaneous respiration, when the action of the brain is suspended, and by the action of the brain when respiration is suspended, such as appears to be the case in amphibious animals, may we not conclude that the influence derived from the respiratory organs and from the nervous system is the same? We have already seen, that if the supply of oxygen be interrupted, these reptiles will perish long before inanition can be supposed to have gone so far as to produce death. An animal may live, therefore, without a nervous system, without a secondary source of power, but cannot live without respiratory organs. I have already given my reasons for believing, that the arterial influence *unites with and becomes a component part* of the animal structure. If, therefore, arterial blood, in its circulation through the system, become venous in consequence of its oxygen being imparted to muscular fibre, must we not suppose that, in its circulation through the brain, it becomes venous in consequence of its oxygen being imparted to nervous fibre? If arterial blood, in its circulation either through the brain or through the system at large, undergo precisely the same change, and if the arterial influence unite with and become a component part of muscular fibre, and impart to that fibre susceptibility of action, must we not suppose that it unites with medullary matter for the same purpose?

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* Med. Jurisprudence.

The quantity of arterial blood sent to the brain is so great in proportion to the size of that organ, that physiologists, upon this fact alone, have regarded the brain as, in reality, a gland, which has the power of elaborating from the blood a new substance, denominated the nervous influence, and as this nervous influence is intended to support the actions of the whole system, the quantity of blood sent to the brain is supposed to be proportioned, not to the necessities of that organ, but of the system at large. Now, as in sleep, those actions of the system by which the nervous influence is expended are much diminished, and, in some instances, (I allude particularly to mental action) altogether suspended ; and as the quantity of blood sent to the brain is not diminished in proportion, nay, in the opinions of some authors, is increased, we must presume that, during sleep, an *accumulation* of the nervous influence takes place in some part of the body. We seem to have positive proof in our feelings, that that principle, whatever it may be, upon which the strength of the body is dependent, is different at different times ; there must, therefore, at one time, be a comparative *accumulation* of this principle. If we find that, by interrupting the supply of the arterial or nervous influences, the power of an organ be absolutely suspended, may we not regard these principles as sources of power, as the principles upon which the power of an organ is dependent? We may presume, therefore, that at one time there is a comparative accumulation of those principles in the different organs of the body. If the arterial influence be capable of being accumulated in muscular fibre, why may it not also be accumulated in nervous matter? Why should nervous matter be an exception to the general law of the system? It seems, then, that the arterial influence is sent to the brain, not only for the purpose of supporting its own individual action, but of being accumulated in that organ, and distributed to the several parts of the system according to their necessities; and therefore the quantity of blood sent to the brain is greater than the size or action of that organ would seem to require.

How is the nervous influence distributed to the several parts of the system? It has been supposed that the action of a nerve is dependent upon its neurilema. "The neurilema, according to our author (Professor Keil,) is not an accidental but essential part of the nerve. It determines their consistence, form, and organization. It supplies the continual waste of medullary matter, which is topically secreted in every part of the nerves by the neurilema; and therefore the nervous power, depending on the quality and quantity of the medulla, is generated in the nerve itself, and does not proceed from the brain as was formerly imagined. No part of the body is more copiously supplied with blood vessels, than the nervous system. The nerves are always closely accompanied by arteries, which send twigs to be distributed throughout their whole substance. They must, therefore, be subject to inflammation; and our author actually found the nerves of a man, who died of typhus fever, and had many violent nervous symptoms, remarkably turgid with blood."* The fibrous structure of medullary matter is readily pointed out by the mode of dissection adopted and recommended by Dr. Spurzheim; and Dr. Darwin says, "The following article evinces, that the organ of vision consists of a fibrous part, as well as of the nervous medulla, like other white muscles; and hence as it resembles the muscular parts of the body in its structure, we may conclude that it must resemble them in possessing a power of being excited into *animal motion*."† The same argument would lead us to conclude, that nervous fibre possesses susceptibility of motion;—and Dr Darwin says, "It is probable that the locomotive muscles of microscopic animals may have greater tenuity than those of the retina, and there is reason to conclude from analogy, that the other immediate organs of sense, as the portio mollis of the auditory nerve, and the rete mucosum of the skin, possess a similarity of structure with the retina, and a similar power of being *excited into action.*"* How is this action excited ? If the impression of light be capable of exciting the action of the retina, why may not the accumulated arterial influence be capable of exciting the action of nervous fibril ? It seems, then, that the arterial influence not only imparts to nervous fibre its susceptibility of action, but may be regarded as its appropriate stimulus. The arterial influence, it will be said, cannot be a source of power and of exhaustion. The arterial influence which unites with and becomes a component part of nervous fibril imparts susceptibility of action ; the arterial influence, which is *accumulated* in nervous fibril, is something distinct from its organization, and therefore may act as a stimulus.

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* Med. Annals. 1798. † Zoonomia.

How is the arterial influence communicated to the several organs of the body through the medium of nerves ? Analogy would lead me to suppose, that nerves resemble arteries in their mode of action; that the arterial influence is communicated to the several organs of the body through the medium of arteries and of nerves in the same manner; and that what has been termed the process of assimilation may be accomplished both by arteries and nerves; and this analogy is strongly supported by the fact, that the action of nerves is usually attributed to their *extremities*.

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* Zoonomia.

Consistently with this general view of the subject I am disposed to regard the ganglia as so many brains, as so many reservoirs of power distributed through the system; and the advantage derived from such an arrangement appears to be that of rendering the organs which they supply more independent of the spinal marrow and brain. Thus the organs of locomotion are subject to an irregular expenditure of the nervous influence, corresponding with the will of the animal. This irregular expenditure will be supplied to a certain extent by the influence derived from the ganglia, and, therefore, the voluntary muscles do not derive their power from the spinal marrow, but from the ganglia. One advantage derived from this circumstance appears to be this: The brain and spinal marrow may be regarded as the great reservoir of power supplying *all* the organs of the body. If an animal, by an ordinary act of the will, could continue his exertions until the functions of an organ became immediately dependent upon the brain and spinal marrow, it could, by an act of the will, derange the whole functions of the system, because it could derange the action of the brain and spinal marrow, upon which those functions are dependent. But the will of the animal is, in a great degree, limited to the ganglia; when these are exhausted, the power of the voluntary muscles is exhausted also. If, in opposition to the warning voice of nature, exertion be carried beyond the feeling of exhaustion, the animal *then* draws upon the spinal marrow for power, and creates disease. There are few organs of the body which are not *indirectly* dependent upon the will of the animal. The action of the stomach, for instance, may at any time be excited by taking food, and the action of the heart by any exertion, because that exertion impels the blood in increased quantity upon that organ. The arterial influence, then, is accumulated in the nerves, for the immediate and ordinary supply of their respective organs; in the ganglia, for the extraordinary supply which the increased action of those organs may require; and in the spinal marrow and brain for the support of every organ in the body; and the nervous influence is assimilated with the several organs of the body by a secreting action of the nervous extremities.

CHAPTER XVIII.

DIGESTIVE FUNCTIONS.

ANIMAL HEAT.

RESPIRATION, circulation, and nervous action appear to be the immediate agents in the generation of animal heat; the two former have long been regarded as the sources of this principle, but the experiments of Mr. Brodie have satisfactorily established an intimate connection between the temperature of the body, and the action of the nervous system. It is unnecessary, in the present day, to assure my reader, that the animal machine is neither a mill, a beer vat, nor a dunghill; and yet the earlier physiologists, in their attempts to explain the phenomena of animal heat, discovered, in their imaginations, many points of analogy between the mechanical action of a mill, the fermenting or chemical action of a beer vat and a dunghill, and the living action of the system. Opinions, grafted upon such views of the animal economy, have, as might have been expected, fallen into decay; and the theory proposed by Dr. Crawford is that which, of late years, has most generally been adopted.

A remarkable connection between the development of the respiratory organs, the corresponding oxygenation of the blood, and the degree of animal heat, may be observed in all classes of animated beings; and this fact naturally led to the idea, that the oxygen might undergo some change, by which it became the source of the heat evolved. The experiments of Dr. Black having established the important chemical facts, that bodies, indicating by the thermometer the *same* degree of sensible heat, often possess in themselves *different quantities* of that principle; and that all bodies, in passing from a gaseous into a fluid, or from a fluid into a solid state, have their capacities for heat diminished, and consequently give out caloric; these facts suggested an ingenious theory, by which the generation of animal heat was supposed to be explained.* The oxygen of the air unites with the blood in its passage through the lungs, but cannot be discovered in that fluid in its natural and gaseous form; it was, therefore, very fairly presumed, that the oxygen, in uniting with the blood, had passed from a gaseous into a fluid or solid state, and that its capacity for heat being thus diminished, the evolution of caloric was the necessary consequence. But a strong objection to this theory was suggested by Dr. Cullen. Why is not the temperature of the lungs, the heat-generating organs, greatly superior to that of the rest of the body? This objection was, in some degree, obviated by the ingenious experiments of Dr. Crawford, who found that arterial blood possessed a greater capacity for heat than venous, and inferred that the superabundant caloric might be contained in the arterial blood, although its *sensible* temperature was not increased; that blood in its circulation through the body, was constantly passing from its arterial to its venous state, by which its capacity for heat was diminished, and caloric evolved in every part of the system. But the experiments of Mr. Brodie have satisfactorily proved, that, if the brain be previously destroyed, oxygen may unite with the blood in its passage through the lungs, and may again, in the course of circulation, be separated from it; in other words, that venous blood may become arterial in the lungs, and return to its venous state in the course of circulation, and yet *no* animal heat be generated. "When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated, notwithstanding the functions of respiration, and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs. When the air respired is colder than the natural temperature, the effect of respiration is not to generate but to diminish animal heat."* The experiments, then, from which these facts are deduced, seem to prove that animal heat is not the effect of any chemical union of the blood and the oxygen of the air, because this union takes place and no animal heat is generated. Neither is it the effect of that process by which arterial blood becomes venous in the course of circulation, because this change is produced in the state of the blood, without the occurrence of such an effect. There is no connection, then, between the generation of animal heat, and the transition of arterial into venous blood.

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* "Dr. Black, whose admirable discoveries relative to heat form so brilliant a period in the history of modern chemistry, found that a quantity of heat, equal to 140 of Farenheit, was required to convert ice into water, although the water still continued at the temperature of 32; and to raise water into vapour required 800 more, although the temperature of steam did not exceed that of boiling water."—*Ellis's Inquiry.*

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* Croonian Lecture.

Consistently with these experiments, animal heat would appear to be the effect of a *living* action, in which the nervous system is the principal agent; and yet the phenomena of vegetable life would lead us to suppose, that heat may be the effect of a *living* action in which the nervous system is not at all concerned. We find that the natural heat of animals corresponds with the perfect or imperfect development of the respiratory organs, and that this heat is increased or diminished in proportion to the supply and expenditure of oxygen. (The apparent exceptions to this rule, which were suggested by Dr. Currie, will be adverted to hereafter.) Upon this principle every increased exertion of the animal system leads to an increased evolution of caloric. It is not easy, with this strong fact before us, to disregard the connection between the supply of oxygen, and the production of animal heat.

Does the oxygen, *in its passage through the system*, undergo any change which may be productive of animal heat, in which change the nerves are chiefly concerned in the superior animals, and not at all in zoophytes and vegetables?

We find from the experiments of Mr. Brodie, that when the circulation was supported by artificial respiration, after the brain had been destroyed, *no animal heat was generated, and no secretion of urine* could be observed to have taken place. Is animal heat a secretion? Dr. Darwin says "These (increased actions of the secerning system) are always attended with increase of partial or of general heat, for the secreted fluids are not simply separated from the blood, but are new combinations. *All new combinations* give out heat chemically; *hence the origin of animal heat*, which is always increased in proportion to the secretion of the part affected, or to the general quantity of the secretion."* Now all new combinations do not give out heat chemically; and the glands, where these new combinations are principally formed, are not warmer than other parts of the system. Dr. Darwin, still adhering to the idea of secretion, thus continues:—"Nevertheless there is reason to believe, that as we have a sense purposely to distinguish the presence of greater or less quantities of heat, so we may have *certain minute glands* for the secretion of this fluid, as the brain is believed to secrete the sensorial power." The existence of these glands being wholly imaginary, I shall pass on to the observations of Dr.

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* Zoonomia.

Currie. "Respiration is a case of compound elective attraction, in which the oxygen gas is decomposed, and enters into new compounds; a part seems to be absorbed by the general mass of blood circulating through the lungs; a part unites with the hydrogen of the venous blood and forms water; another part with the carbon of this blood, and forms carbonic acid gas. In the two first cases the language of the text applies strictly, but in the last case the heat extricated does not arise from the oxygen becoming concrete, but from its entering into the composition of a gas, which has a less capacity for caloric than itself. The heat disengaged in these different processes unites with the blood, which, in losing its hydrogen and carbon, is converted from venous to arterial, and has its capacity for caloric increased. Thus inflammation in the lungs is prevented. But the arterial blood, in the course of circulation, again absorbs, by little and by little, a portion of hydrocarbonat, parting with caloric in the same proportion; hence the uniformity of temperature throughout the body. This seems the modern theory of respiration, which, it must be confessed, is not without its difficulties."*

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* Currie's Reports.

The theory suggested by Mr. Ellis, though varying in some particulars, appears to rest upon the same fact. "We have seen reason to conclude, that the inspired air is decomposed in the bronchial cells of the lungs, and that all its oxygenous portion which disappears is converted into carbonic acid by carbon emitted from the exhalant surface of these organs. During this gradual conversion of the oxygen gas, a quantity of specific caloric, much greater than what is necessary to maintain the elasticity of the carbonic acid that is formed, is necessarily set free; and to this excess of heat, thus constantly liberated in the lungs by the decomposition of the air, do we look as the source of that superiority of temperature above the surrounding media, which man and other animals, under every vicissitude of climate, are enabled to exhibit and maintain."* But, in the experiments of Mr. Brodie, the air was decomposed in the lungs, and no animal heat generated. And yet the fact, that the temperature of animals corresponds with the perfect or imperfect development of the respiratory organs, and with the quantity of air which they respire and decompose in a given time, seems to imply that the oxygen of the air is the principal agent in the production of animal heat. I have endeavoured to shew, that oxygen unites with the blood in its passage through the lungs, for the purpose of being assimilated, of being *incorporated* with the system. "That the oxygen contained in the atmosphere furnishes the largest portion of the *pabulum* of life, is now scarcely doubted, respiration being a process in which it passes from its *gaseous* to its *concrete* state, giving out a portion of the heat it held in vapour, in the same manner as steam, in its conversion into water, gives out a portion of its heat."* "For this office it appears, from the observations of Berthollet, that oxygen gas is peculiarly fitted, since no other substance gives out *so much caloric* by the changes of constitution which it suffers."† If, then, oxygen, by uniting with the blood, or with the carbon excreted from it, be supposed to pass from a gaseous to a concrete state, such a change must be admitted to take place, when it becomes *incorporated* with the *solids* of the body. Oxygen, then, would appear to give out caloric, *not in the lungs particularly*, but in every individual part of the system; and animal heat would appear to be the result of that process, by which the air we inspire *is assimilated*, *is incorporat-*

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* Inquiry.

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* Inquiry. † Ibidem.

ed with the solids of the body. There is no process, except that of assimilation, by which the arterial influence is incorporated with the system; and I have endeavoured to shew, that this process is accomplished by the extremities of arteries *and of nerves*; and we have also seen that the action of the secreting extremities of arteries is, in a peculiar manner, dependent upon the nervous influence, upon the secondary source of power. We may now explain why, in the experiments of Mr. Brodie, the action of the nervous system being suspended, no animal heat was generated. The process of assimilation was suspended also. As the assimilation of oxygen with the system is, in health, proportioned to the expenditure, and as the expenditure is proportioned to the actions of the system, it follows that the degree of animal heat will, in health, always correspond with the degree of action. This rule is equally applicable to disease, but will be more particularly adverted to hereafter. Every action of the body, whether partial or general, is a source of animal heat, because it is attended with an increased expenditure of oxygen, and a corresponding supply through the medium of the arterial or nervous influences; and because the oxygen thus supplied is assimilated with the system, and its capacity for heat diminished. In disease, the supply of oxygen may not correspond with the expenditure, and then the degree of heat will not correspond with the degree of living action; but even in disease, the heat will always correspond with that *peculiar* action by which the oxygen is assimilated with the system.

If caloric and electricity be the same principle in a different state of modification (and such appears to be the general opinion), it probably amounts to the same thing, whether we say that the arterial and nervous influences have their capacities for heat or for electricity diminished. Oxygen may unite with the system for the purpose of supporting its organization and action, and its electricity be evolved, and become the source of animal heat. Such an explanation may be adopted without at all interfering with the fact, that heat is the consequence of that process by which the arterial and nervous influences are assimilated with the system.

It is by no means improbable, that the assimilation of our food, which is the conversion of a fluid into a solid, may be attended with an evolution of caloric. "Caloric is received into the system by other processes also, particularly from the food and drink in the state of digestion (I should say of assimilation with the body), and evolution of it into active heat appears to be modified by the living power on its peculiar principles."* The evolution of animal heat may thus be regarded as the result of that process, by which the food we take and the air we inspire are incorporated with the system.

Dr. Currie has recorded an exception to the general rule, that the degree of animal heat will always correspond with the supply of oxygen. There must, he observes, be a great expenditure of heat in a cold bath; but, after the shock of immersion, the pulse and respiration are not quickened in proportion to the heat which we find to be generated. The same author, in another part of his valuable work, says, "That the action of cold on the living body is, under certain circumstances, *stimulating*, is maintained in note, p. 73, to which the reader is referred; and this stimulation depends on the sensation by which the application of cold is attended."* There can be little doubt that cold acts, in the first instance, as a sedative to the system, and that it is *indirectly* stimulant; that is, a certain reaction of the system is the consequence of a sedative effect produced by the application of cold. Now this re-action may be supported for a certain time without a corresponding supply of oxygen *through the medium of the lungs,* because there is a supply, a reservoir of oxygen provided, for the peculiar purpose of supporting living action under all similar circumstances, in the nervous system; I mean, that whenever the expenditure of oxygen exceeds the supply through the medium of the lungs, that expenditure is supported by the nervous system. In the experiment mentioned, animal heat would appear to have been supported by the assimilating action of the nerves.

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* Currie's Reports.

Heat is produced by the germination of seeds. In the process of malting, grain improperly kept is often known to take fire, and the fact has been proved by experiment. "Some peas, which had been steeped in water 36 hours, were placed in a small glass jar, standing in a room at temperature 63. In this situation they remained till signs of germination appeared, when the bulb of a very delicate thermometer was plunged into the midst of them, which had previously been compared with another standing beside it. The standard thermometer remained stationary for more than an hour, but the one that was plunged among the peas, although at first 0.4 below the former, rose in 20 minutes 0.2 above it, and in 40 minutes was 0.4 above it; but, at the end of half an hour, it had again fallen to 0.2 of superiority only."* In another experiment the thermometer rose, in half an hour, one degree, and sunk again to 0.5 The *variation* in the temperature cannot, I conceive, be attributed to any other cause than the living action of the seed. We have here an example of heat being generated by a living action in which nerves are not at all concerned. That the process of assimilation is carried on in seeds we have evident proof in their growth; and analogy would lead us to suppose, that the air is subjected to the same process, by which its capacity for heat is diminished, and that principle evolved.

Heat is produced by vegetation. The observations of Mr. Hunter upon this subject have been fully confirmed by subsequent experiments, and particularly by those of M. Hubert. But this author observed, "That if the spadices were closely bound with a cloth dipped in oil, grease, or tallow, no increase of temperature took place."† There is no secondary source of oxygen in vegetables, that is, there is no nervous system, and therefore the degree of heat will always correspond with the quantity of oxygen inspired and assimilated with the system.

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* Ellis's Inquiry. † Ibidem.

In cold-blooded animals, such as frogs, lizards, &c. the animal temperature scarcely exceeds that of the surrounding medium. We find that their actions are sluggish, and that they expend oxygen slowly ; and, therefore, that process by which the arterial or nervous influence is assimilated with the system, for the purpose of supporting living action, is, comparatively speaking, imperfect. Insects are usually denominated cold-blooded animals, the temperature of caterpillars scarcely exceeding that of the surrounding atmosphere ; but a thermometer plunged into a bee-hive will indicate a degree of heat superior to that of man. Do we not, in the sluggish inaction of the caterpillar, and the busy labour of the bee, discover that difference of living action, which implies a difference in the quantity of oxygen expended and assimilated ?

The temperature of fishes scarcely exceeds that of the medium in which they live. We have no means of ascertaining what is the temperature of a fish in its active state, or what is the quantity of oxygen which it expends; but when we consider the rapidity with which water absorbs heat, it is probable that the quantum of heat generated greatly exceeds that which is indicated by the thermometer. "That fishes part with this excess of heat to colder bodies which surround them, we learn also from the experiments of the same author (Mr. Hunter.) He put two carp into a glass vessel with common river water, and then placed the vessel in a freezing mixture; and as the water in the vessel did not freeze fast enough, snow was added, so as to render the whole thick. The snow round the carp melted, and more being added it melted also, till at last *he grew tired of putting it in*, and left them to freeze by the joint operation of the freezing mixture, and the natural cold of the atmosphere; at length they froze, but did not, when thawed, recover life with flexibility; but that both snakes and fishes, after being frozen, have still retained so much of life, as when thawed to recover vital action, is a fact, says Mr. Hunter, so well attested, that we are bound to believe it."* According to the experiments of Dr. Black, a quantity of heat, equal to 140 of Farenheit, is required to convert ice into water. This quantity, then, was given out by the carp, until the system was so exhausted, that no *re-action* took place; and then the freezing mixture was of itself sufficient to reduce the temperature of the fish below its natural standard. In those snakes and fishes which recovered their vital action after being thawed, we may suppose that all re-action ceased before the organization had undergone a change incompatible with the continuance of life. They are precisely in the state of animals which become torpid naturally.

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* Ellis's Inquiry.

We are told by Dumas that the temperature of amphibious animals is but little superior to that of the medium in which they live. But when we consider that water absorbs heat rapidly, we shall find that these animals are occasionally placed under very peculiar circumstances. When living in that element which most rapidly absorbs heat, viz. the water, they are called upon to generate an excess of heat, at a time when they are absolutely cut off from the atmosphere, its principal if not its only source. Whence is this heat derived *during the suspension of respiration?* How is oxygen assimilated with the system? We cannot suppose that the excess of oxygen is derived from the blood, because that, even when in direct communication with the atmosphere, is imperfectly oxygenated. Must we not look to the nervous system as the secondary source of oxygen, as the medium by which oxygen is supplied for the support of living action, and the generation of animal heat?

It has been found by experiment that birds expend, in a given time, more oxygen than any other animal of the same size; more oxygen, therefore, is assimilated with the system, and the temperature of birds is greatly superior to that of man.

Animals in a state of torpidity produce no change in the air. There is no expenditure and, consequently, no assimilation of oxygen, and, therefore, no animal heat is generated. There are many experiments which seem to be in opposition to this fact; but I am disposed to think that the experiments themselves, by exciting living action, produce a degree of temperature which does not belong to a state of absolute torpidity. "Dr. Jenner, whose discovery of vaccination has conferred so much honour on himself, and such incalculable benefits on mankind, at the request of Mr. Hunter, examined the temperature of a hedgehog *in its torpid state.* In an atmosphere of 44 its temperature was, *in the pelvis,* but 45, and at the diaphragm 48.5; and when the atmosphere was reduced to 26°, the heat of the hedgehog *in the abdomen* did not exceed 30."* How was it possible *to get at* the pelvis, the diaphragm, the internal part of the abdomen, without exciting such an action as would produce heat?

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* Ellis's Inquiry.

It has been supposed that different parts of the animal frame possess different degrees of temperature. As the body is surrounded by a medium colder than itself, caloric will necessarily be given out from its surface; but the observation is intended to apply to different organs which are equally remote from the surface, and whose temperature is equally influenced by the external atmosphere. According to the experiments of Mr. Hunter, the temperature in the right side of *the heart of a dog* was 101, and according to Mr. Coleman 98. How was the temperature ascertained? "But on *making an aperture* into the left auricle, *and thrusting the bulb down to the ventricle*, the mercury fell to 97, and on placing it *in the same manner* within the right ventricle, it rose above 98."* Was no *action* excited by removing the sternum, by piercing the pericardium, by thrusting the bulb of a thermometer into the ventricles of the heart? Was *no action* excited which could influence the temperature indicated? It is, I think, clear, that the effects of the experiments themselves have been too much disregarded, and have led to very erroneous ideas upon the subject of the *natural* temperature of animals.

The animal system possesses the power

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* On Suspended Respiration.

of resisting cold. What are the most effectual means of preserving the temperature of the human body under such circumstances? Clothing, by which the rapid expenditure of heat is counteracted, and exercise. A stimulus of any kind will answer the same purpose, so long as the stimulus continues to excite action, or rather that peculiar action by which oxygen is incorporated with the system. Connected with this part of my subject is an observation of Mr. Hunter's, that a frog becomes frozen in a cold mixture of 31, and cannot be re-animated by heat, but will live in air of a much lower temperature; " a point which Mr. Hunter does not pretend to determine, not knowing the difference between the effects of a natural and artificial cold." A cold air contains more oxygen in a given bulk, and thus, by one of those laws of nature which need only be known to be admired and venerated, a cold air may be said to contain more heat in itself than a warm air; that is, it contains more of that principle which, by a living action of the system, is capable of being converted into heat. An animal exposed to the influence of a freezing mixture, and to an atmosphere of the same temperature, is placed under very different circumstances; as, in the former instance, the quantity of oxygen inspired bears no relation to the degree of cold applied, but in the latter is proportioned to it.

The animal body is said to possess the power of resisting heat; that is, of preserving, under certain circumstances, a temperature below that of the surrounding medium. The fact is supposed to be established by the experiments of Du Hamel and Tillet, in France; and of Dr. Fordyce and Sir C. Blagden, in England. It is not, in reality, connected with this part of my subject; but I may observe, that when the body is exposed to an atmosphere of 224, as in the experiments of Dr. Dobson; and of 260, and even of 280, as in the experiments of Sir C. Blagden and M. Tillet, there are some important circumstances to be taken into consideration. The inspired air contains less oxygen in a given bulk, and the actions of the system, by which alone that oxygen can be assimilated and become the source of animal heat, are impaired, probably suspended. We see, therefore, why the body itself should generate less heat; but why the animal system should become a non-conductor of that heat which is external to it, I am at a loss to conceive; and as the fact seems to rest very much upon the *sensations* of the individual, which, under such circumstances, cannot be regarded as very accurate thermometers, I do not consider the fact as fully established. Animal heat, then, is always proportioned to the general or local action of the system, because the quantity of oxygen expended and assimilated will, in health, always correspond with the degree of action. The apparent exceptions to this rule are all connected with pathology; but, regarding the nervous system as a secondary source of the arterial influence, I am disposed to think that these exceptions will rather tend to confirm than to invalidate the theory proposed. The air, then, is digested in the lungs; in other words, is decomposed into its separate elements, azote and oxygen; the former, the excrementitious principle, is, as soon as convenient, eliminated from the system; the latter, the nutrient principle, unites with some part of the blood, probably the serum, and constitutes a new product denominated the arterial influence. A portion of this influence unites with every organ of the body which has an active function to perform, for the purpose of supporting its susceptibility of action, and the remainder is accumulated in the nervous system. This influence is conveyed, through the medium of the circulation, to every part of the body, for the purpose of being incorporated with it. It is capable of being accumulated in the several organs of the body, and particularly in the nervous system; and is there accumulated for the purpose of being conveyed to, and assimilated with, the several organs of the body, according to the necessities or the will of the animal ; and the process of assimilation is accomplished by the extremities of arteries *and of nerves.* It cannot be supposed to be incorporated with the solids of the body without passing into a concrete state ; and this state implies that, in the process of assimilation, its capacity for heat, or for electricity, is diminished, and that caloric, in one of these states of modification, is evolved. Having contributed to the support of living action, it becomes excrementitious, is separated from the organization, is taken up, in union probably with the excrementitious fluids, by the lymphatic absorbents, is re-conveyed to the blood for the purpose of being eliminated from the system, and may again be discovered in the different excretions. There are many pathological facts which lead me to suppose that the function of the thymus gland is intimately connected with that of the brain. Does it counteract too great an accumulation of the nervous influence ? does it produce this effect in the infant? does a diseased action of the thymus gland produce idiotcy, by preventing a due accumulation of the nervous influence in the brain? does the diseased action of the thymus gland, at particular periods of life, indicate too great an accumulation of the nervous influence in the brain?

Having already traced our food through the different processes by which it becomes assimilated with the system, and is ultimately eliminated from it, I shall briefly state, that the different processes of digestion to which it is subjected may be influenced by a principle derived from the action of the organs themselves, and that this principle may be the nervous influence. This subject will necessarily be re-considered in a future part of these observations.

CHAPTER XIX.

Sensitive Functions.

In the term "Sensitive Functions" I include all those living actions, which (through the medium of that intercourse between mind and matter, which all acknowledge but none comprehend) are attended with the consciousness of the animal. I have expressed it as my opinion, that the nervous system has but one function to perform, viz. that of communicating what has been termed the nervous influence to the different organs of the body, for the purpose of supporting their organization and action; and I have given my reasons for supposing that the arterial and nervous influences are, in reality, the same things, and contribute to the support of living organization and action in the same manner.

It has been assumed as a fact, that the brain is the organ of the mind; but we learn from experience and observation, that considerable portions of the brain may be destroyed, and the mind continue as perfect and active as it was before. The mind, therefore, is something distinct from the organization of the brain, considered as a whole. Physiologists have endeavoured to find out some circumscribed part of the brain, which might be regarded as the residence of this something which we call mind. They naturally looked for a central point with which all parts of the brain appeared to communicate, and such a point being discovered in the commissures of the brain, these were fixed upon as the residence of the mind. Descartes carried his views still farther, and selected the pineal gland as the peculiar organ in which the mind took up its abode. But it was soon discovered that the mind was impaired by disease, which had produced a change in the organization of other parts of the brain, but had not at all affected either the commissures or the pineal gland. But the brain is the organ of the mind, and therefore, it is said, in the brain the mind must have its residence. The latest speculation upon this subject has pretty fully convinced Physiologists that the mind will never be traced to any central point of the brain. We are told, by Professor Soemmering, that the mind resides *in the halitus* of the ventricles. It would be difficult to prove that it does not, but the *onus probandi* rests, with the Professor, and I am inclined to think that such a task will be too great for the abilities even of a Soemmering. In direct opposition to these views is a modern theory suggested by Dr. Gall, illustrated by Dr. Spurzheim, and adopted by some of our most able physiologists. It is supposed, not only that the brain is the organ of the mind, but that particular parts of the brain are the particular organs upon which particular *faculties* of the mind are dependent. Thus the memory is connected with one circumscribed portion of the brain and the imagination with another.* There seems to be a very simple mode of ascertaining the truth of this theory. If that individual part of the brain, upon which an individual faculty of the mind is dependent, be destroyed by disease, it follows that that individual faculty of the mind must be destroyed also. But we have innumerable examples to the contrary. It is clear, that without a satisfactory explanation of this fact the theory must fall to the ground. The fact is admitted and thus explained :—There are two kidneys in the body, and it has been found, that when the function of one is suspended by disease, the function of the other is increased, so as, *in some degree*, to compensate for the deficiency; in other words, one kidney performs the function of both. In the same manner each faculty of the mind is supposed to be dependent upon two corresponding portions of the brain; and when one is destroyed the other performs the function of both, *and it performs it equally well.* I much doubt whether any analogy between the two corresponding portions of the brain and the two kidneys can be established in this respect. But why should we go to the kidneys for an analogy, when the brain itself is so much nearer home? If one side of the brain be injured, the muscles connected, through the medium of their nerves, with the injured portion of the brain, become paralytic. Why does not the other side of the brain perform the function of both? Has nature been more anxious to provide for *that faculty* of the mind which may instigate a man to commit murder, than for his locomotion, which is almost essential to his existence? The *fact*, then, which ought to establish the theory, is in direct opposition to it; and the hypothesis, for I can give it no other name, rests upon an analogy, which, if admitted, is far from being correct *in the material point*, and is contradicted by an analogy, derived from an acknowledged function of the brain itself. Craniology, I may observe, is by no means of recent date. The seeds have long been sown, and perished for want of those principles which are essential to the support of living action. Thus M. De Saint Hilaire, in speaking of the brain, says, "Regius et Hogelande veulent, que dans les detours ou anfractuositez de la partie cendrée soient conservées les especes *qui* FONT *la memoire*, de sorte qu'ils se recontrent dans les hommes en plus grand nombre, et plus diversifiez que dans les autres animaux, a cause qu'ils ont *plus de memoire*. M. Duncan croit aussi, que c'est dans la substance cendrée, que l'ame *se ressouvient des choses*. M. Duncan etablit *la siege de l'imagination* dans cette partie moëlluse du cerveau, et l'unique raison qu'il en rend est, que *cet endroit* est comme *le reservoir des esprits*, et par consequent *le lieu*, ou les ondulations sont plus sensibles à l'ame."* The mind, considered as a whole, has never been traced to any central point of the brain; and I think it equally clear, that no particular faculty of the mind has ever been traced to any particular part of that organ. The facts upon which the doctrine is founded, are, in my opinion, wholly imaginary; and the facts, which are in direct opposition to it, are, *in my opinion*, but ill explained, by substituting ingenuity for argument, subtlety for legitimate reasoning, and analogy for matter of fact. The mind, then, whatever it may be, has never been traced to any particular part or parts of the brain.

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* I purposely avoid the technicalities and the phraseology of this modern doctrine.

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* L'Anatomie du Corps humain par Le Sieur de St. Hilaire, 1698.

It has been questioned, upon very strong grounds, whether the mind be confined to the brain. There are numerous facts, arising principally from the actions of a decapitated animal indicating volition, and from the actions of some animals, in which no brain has yet been discovered, being obviously connected with this mental faculty, which seem to imply that the brain is *not* the exclusive organ of the mind; " on the other hand, that the intervention of the brain is *not* necessary to the association of certain muscular movements, from slight irritation, on a distant part, *such as* USUALLY *produces sensation*, is evident from the case of a fœtus, in which there was not the smallest vestige of cerebrum or cerebellum, but which, even twenty hours after birth, moved up its knees when the soles of its feet were tickled, sucked my finger when introduced into its mouth, and, a few hours before, had passed urine and fæces, *and swallowed food.*[*]" Dr. Spurzheim says, "All such phenomena observed in insects, reptiles, birds, quadrupeds, and even in man, are the result of irritability *without consciousness.*"[†] If those actions, which lead us to infer the agency of the will, are found to occur when there is no brain to which that will can be referred, the first idea which naturally suggests itself is this: that will may exist independently of the brain. Now there is no one fact (I might add, or semblance of a fact) which justifies us in asserting, "that all such phenomena are the results of irritability *without consciousness;* but the phenomena which we observe are the very phenomena from which, under ordinary circumstances, we infer the consciousness of the animal. We have, therefore, a *strong* argument in favour of the opinion, that these actions are connected with the will of the animal, and we have no argument in support of the opinion that they are not. We may, indeed, assume the fact, that mind cannot exist without brain, and infer that, consistently with this fact, the will, being a faculty of the mind, *cannot* exist when the brain does not. But this argument rests entirely upon the first assumption; and the assumption itself is in direct opposition to the fact, that, in the inferior animals, we admit the agency of the will, although no brain has ever been discovered to which that agency can be attributed. Professor Cuvier says, "The *echinus*, or *sea eggs*, are encrusted by a complete calcareous skin, the surface of which is covered with tubercles, disposed in a very regular manner. Moveable spines of various shapes and sizes are articulated to these tubercles. It is very difficult to discover the fibres by which the spines are moved *at the will of the animal*."* The voluntary muscles derive their nerves from the spinal cord. There is no difference in the structure of the brain and spinal cord, at least none which applies to the point in question; and, therefore, we might presume that the functions of the brain and of the spinal cord are the same. The nerves resemble, in structure, the brain and spinal cord; and, therefore, we have great reason to suppose, that they resemble the brain and spinal cord in function. There is nothing in the structure of the brain which points it out as the residence of the mind; and, therefore, *as far as the structure* of the nervous system is concerned, there is no reason why the mind may not, with equal probability, be regarded as resident either in the brain, spinal cord, ganglia, plexusses, or nerves. There is nothing in the structure of the nervous system, considered as a whole, which points it out as the residence of mind; and, therefore, as far as structure is concerned, there is no reason why the mind may not exist in muscular fibre, or in any part of the living organization, as well as in the brain or nervous system. It has been said that our feelings are conclusive upon this point, that every person is conscious, that when he thinks he thinks with the brain; in other words, he feels that there is a connection between that mental action denominated thought, and some action of the brain. But how does this argument apply to another function of the mind—to sensation? If the finger be wounded, our feelings refer the sensation, not to the brain, but to the finger; it follows, therefore, that if our feelings are conclusive upon this point, the mind does not reside in the brain, but in the finger.

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[*] Parry's Elements. [†] Physiognomy.

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* Comparative Anatomy.

The facts which have led to the conclusion, that the brain is the exclusive organ of the mind, are principally derived from the pathology of the nervous system. If the nerve supplying a muscle be divided, that muscle becomes *paralytic*; the *action* of a muscle, therefore, is dependent upon some property communicated by its nerve. But the muscle has not only lost the power of being excited into action, but may be irritated mechanically or chemically, without exciting the consciousness of the animal. Now this fact may be considered in two points of view. The loss of sensation is dependent upon the diseased state either of the muscle or of the nerve. The latter opinion has been adopted, and muscular fibre has been regarded as the organ of motion, and nervous fibre as the organ of sensation. If nervous fibre be the organ of sensation, no part can be sentient without nerves. The skin is regarded as the most sentient part of the body. The most minute point of the skin will, upon being irritated, convey to the animal a certain feeling of which he is conscious. Now the fact that every part of the skin possesses nerve is very far from being proved. Mr. Bell, in speaking of what he has denominated the symmetrical nerves, says, " they are all exquisitely sensible, and the source of the common sensibility of the surfaces of the body; *when accurately represented on paper*, they are seen to pervade every part; no part is without them; and yet they are symmetrical and simple as the nerves of the lower animals."* We have not been engaged in a twenty-five years' war, without learning the difference between an army *on paper*, and an army *in the field.* Let us examine the most accurate dissection of nerves that was ever accomplished by the labour and science of man, and we shall at once see the difference between the nerves which have been *seen*, which have been demonstrated, and those which are *imagined* to exist. It will be said that we have no right to assume the fact that nerves do not exist because they cannot be demonstrated. But it is surely a much greater assumption to say that, under such circumstances, they do exist. Nerves, it is said, are the *exclusive* organs of sensation, and therefore, every part of the skin being sentient, every part of the skin *must* be endowed with nerves. But if the fact, that nerves are the exclusive organs of sensation, remains to be proved, the argument falls to the ground. I question the fact. Where is the anatomist who will be bold enough to say that I *cannot* question the fact, because he will prove that no single point of the skin can be irritated, without at the same time irritating a nerve?

Admitting that nerves exist in every part of the body, and that they are the exclusive organs of sensation, let us pursue the subject a little farther. The nails, in their healthy state, are insentient; they may be cut or lacerated, without conveying to the animal any sensation. The nails, therefore, are not endowed with nerves. But the nails, in their diseased state, become acutely sentient. Are they sentient without nerves? Can we suppose that nerves are generated by disease, for no other apparent purpose, than that of enabling the animal to be conscious of suffering? Bone, in its healthy state, is insentient; but every part of bone is capable of becoming acutely sentient in disease. Are we prepared to say that every part of bone contains nerve? The organization of bone is absolutely incompatible with the idea. We find, for instance, that a certain degree of pressure will suspend the function of a nerve. How can a nerve be supposed to penetrate the solid structure of bone, without being subjected to a degree of pressure which is incompatible with its function? Bone is insentient in health, but becomes acutely sentient in disease. Are nerves generated by the disease?—For what purpose? for the apparent purpose of enabling the animal to be conscious of pain. Such disease is of rare occurrence. Are the bones supplied with *nerves*

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* Philosoph. Transact. 1821.

of sensation, in the great majority of cases, for no purpose at all? If, according to the present theory, the nerves of sensation and of motion are distinct, the former must be supposed to exist in bone, in the great majority of cases, for no purpose at all; or to be generated by disease for no other purpose than that of enabling the animal to be conscious of pain. Nerves, then, considered as organs of *sensation*, cannot be supposed to pervade every part of bone, without being imaginary in existence, distinct from other nerves in their mode of action, and apparently useless in function.

Admitting that nerves are the exclusive organs of sensation, how is that feeling, termed sensation, supposed to be excited? A certain impression is made upon some part of the body, (upon the skin for instance); every part of the body is assumed to possess nerve, and therefore that impression is necessarily made upon the extremity of a nerve. The fact as to the ubiquity of nerves is assumed. This impression produces *a certain change* in the extremity of the nerve. This fact may be granted, provided the impression be made upon the nerve itself, but rests upon the first assumption. The change which takes place in the extremity of the nerve is communicated to the brain. This fact is wholly imaginary, and is in opposition to the acknowledged function of nerve, which is from the brain to the extremity. A corresponding change takes place in the brain itself. This fact is wholly assumed. The mind is sensible of the change in the brain, but is not sensible of the change in the extremity of the nerve. Why should the mind be capable of observing a change in one part of the nervous system and not in another? The mind is sensible of the change in the brain, but the animal is not; the animal is sensible of the change in the extremity of the nerve, but the mind is not. What is the difference between mind and animal, when this latter term is used to imply consciousness?

If a nerve supplying a muscle be divided, that muscle may be irritated without exciting sensation. Now, regarding the nerve as simply the organ of power, as conveying to organized matter its susceptibility of action, it follows, that by dividing the nerve, we have destroyed the capability of action. I have already said, that the immediate stimulus of every organ is a change in the relative situation of its particles. Sensation is always the effect of an impression made upon, that is, of a stimulus applied to, some part of the body. But if this impression be made upon any part which has lost its susceptibility of action, upon a compressed nerve for instance, no action will take place, and no sensation will be experienced; the action of the organ, therefore, upon which the impression is made, must be regarded as a necessary link in the chain of those actions which ultimately terminate in sensation. By dividing the nerve supplying a muscle, we have deprived the latter of its power of action. The loss of sensation is not dependent upon the circumstance that the divided nerve cannot communicate with the brain, but upon the circumstance that a paralytic muscle cannot be excited into that peculiar action which is a necessary link in the chain of actions which lead to sensation. It has been said, that a paralytic muscle retains its susceptibility of action unimpaired, and the fact is supposed to be proved by the circumstance, that contraction may be excited in a muscle after its nerve has been divided. Dr. Philip says, that a voluntary muscle is paralytic, not in consequence of any change in its susceptibility of action, but in consequence of the nervous influence, its sole and appropriate stimulus, being intercepted. Why, then, is an involuntary muscle paralytic when its appropriate stimulus is present? Why are the sphincter muscles relaxed in apoplexy? Admitting that action may be excited by a foreign stimulus applied to a paralytic muscle, it is far from being proved that the animal is unconscious of that action. And even supposing this to be proved, a paralytic muscle may not be capable of being excited into that peculiar action which is necessary to sensation. I conceive, then, that a paralytic muscle is void of sensation, not because the nerve is incapable of transmitting a certain change to the brain, but because the diseased organ, which that nerve supplied, is incapable of being excited into that peculiar action which is necessary to sensation. Every part of the body which is capable of being excited into action, is capable of becoming the organ of sensation. Why, then, are bones, in their healthy state, insentient, and acutely sentient when diseased? I have already endeavoured to shew, that life does not impart susceptibility of action; that a living organization does not necessarily possess the capability of being excited into action. Bones, therefore, may be endowed with life, but not possessing, in their healthy state, susceptibility of action may not possess susceptibility of sensation. Bones, in their healthy state, would derive no apparent advantage from susceptibility of action. They are passive agents (if I may use the expression) in the animal economy; their function is wholly mechanical. The organization of bone, therefore, is not that peculiar organization which possesses susceptibility of action; but if this organization be altered by disease, susceptibility of action, and, therefore, susceptibility of sensation, may be acquired. Pain is thus the natural consequence of this diseased action, and not of any organs provided for the sole purpose of being the instruments of pain. If the nerves of sensation and of motion are distinct, bones, being capable of sensation, must be supposed to possess *nerves of sensation;* and as bones are only sentient when diseased, these nerves, in the great majority of cases, must be absolutely useless and inactive, and, when called into action, injurious to the animal.

It is singular that the nervous system should have been regarded as the sole and exclusive organ of sensation, in opposition to the well-known fact, that nervous matter is less sensible than most other parts of the body. Some authors have contended that the brain is absolutely insensible, and it is certain that considerable portions of the brain have been removed with much less pain to the patient than is consistent with the idea of nervous matter being the organ of sensation. Conceiving that the action of a nerve depends upon its neurilema, and that nervous matter is itself inactive, I am disposed to agree with those authors who deny the sensibility of nervous matter altogether. I have endeavoured to shew that the action of a nerve depends upon its neurilema. Irritation of the neurilema, by exciting action, will excite sensation. It is probable that the neurilema may be more susceptible of action than most other parts of the body, and that nerves may be more readily excited into that peculiar action which leads to sensation. I do not, therefore, mean to contend that nerves may not become organs of sensation, but that they are not exclusively so. I conceive, then, that whenever an organ is deprived of its sensibility by the nerve or nerves of that organ having been divided, compressed, or in any way injured, the loss of sensation is dependent upon the circumstance, that that organ no longer possesses the capability of being excited into action; and, therefore, a necessary link in the chain of actions which lead to sensation is wanting.

But it often happens that these actions do positively take place without sensation being excited. Impressions, for instance, are constantly made upon the organs of vision, hearing, taste, smell, and touch, without sensation being the result of these impressions. The organs are in a healthy state, and, therefore, we must conclude that the appropriate stimulus applied to its appropriate organ produces that change which, under other circumstances, leads to sensation. Convulsions occur in apoplexy, but the individual is wholly unconscious of them.—Why? Because the mind is not in a fit state to perceive them. Why does pressure upon the brain produce that state of the mind? This is the very circumstance, it will be said, which has induced physiologists to refer all sensation to the brain. Pressure upon a nerve suspends its action, and, therefore, we presume that pressure upon the brain produces the same effect. But the mind is in precisely the same state when there is no pressure; when there is a want of the natural pressure upon the brain; in fainting for instance. The mind is in precisely the same state of inactivity, whenever too much or too little blood is sent to the brain. We have no difficulty in perceiving how a deficiency of blood may deprive the brain of its susceptibility of action, and how a surplus of blood may, by pressure, lead to the same effect. They both produce a change in its organization which is incompatible with its function. How does a suspension in the function of the brain interrupt all consciousness? How is the brain the organ of the mind? The mind is altogether an imaginary organ. I do not mean imaginary as to its existence, but as to its situation, its capabilities, its mode of action, its properties, &c. The mind has no properties which are capable of being perceived by any of our senses, and, therefore, the mind has no properties which are capable of being conceived by the imagination. We admit the existence of mind, because our feelings tell us plainly, that a certain something, which we call mind, which we have agreed to express by that term, does exist. Mind, therefore, is imaginary, not as to its existence, but as to its properties, its capabilities, &c. It exactly corresponds, in this respect, with what has been denominated the vital principle. We infer the presence and agency of these principles, because we perceive certain effects which must have certain causes, and have agreed to express these causes by the terms—vital principle and mind.

I have said that the mind is an imaginary *organ*. It here becomes necessary briefly to mention, what I conceive to be the distinction between the soul and the mind. The soul is, I conceive, like the vital principle, wholly independent of matter for its *existence*, but altogether dependent upon matter for its *union* with the body. When matter undergoes a change which is incompatible with this union, in other words, when matter dies, the soul is separated from it, and maintains a separate and independent existence. "God breathed into his nostrils the breath of life, and man became a living *soul*." If we wished to say that soul is independent of matter, could we possibly express the fact in stronger terms? Matter existed in an organized state without soul, before that matter was animated by the vital spark. The union of the living principle with matter enabled that matter to become the residence of the soul; when matter returns to that state in which it existed before it was animated by the vital principle, it returns to a state which we have already seen to be incompatible with the co-union of soul and body, and the former is then disunited from the latter, and maintains a separate and independent existence. But the mind may be a property of matter itself, altogether dependent upon matter for its existence and its action. The materiality of the mind does not at all involve the materiality of the soul; they are distinct principles; the former being altogether dependent upon matter for its existence and action, and the latter being wholly independent of matter for its existence, and only dependent upon matter for its union with the body. The mind, then, is a property of living matter, an imaginary organ capable of being acted upon through the medium of the body. How is the brain the organ of the mind? How is the nervous system the organ of voluntary motion? The nervous system imparts to voluntary muscles a certain influence which we know to be necessary to their action, because, if that influence be interrupted, their action ceases. In the same manner the influence of the brain is necessary to the action of the mind. But we have no more reason to say that the brain is the organ of the mind, than that nerves are the organs of voluntary motion. We have already seen that the action of every organ in the body is dependent upon some influence derived from another. Why should the mind be an exception to this rule? We have every reason to say that it is not an exception. In fainting, for instance, we have evidence that the action of the brain is diminished, in consequence of the due supply of blood not being transmitted to that organ; and we have evidence not only in the state of the mind, but in that of the body. The action of the mind, then, is, like that of other organs, dependent upon the arterial influence ; because, the arterial influence being interrupted, the action of the mind ceases, and the arterial influence being restored, the action of the mind is revived. The mind, then, exactly corresponds with the other organs of the body in this respect, why should it not correspond with other organs of the body in its laws of action ? in other words, why should not its organization and action be dependent upon some influence derived from some other organ? The voluntary muscles may be thrown into action by the will of the animal, because, the nervous influence having been placed at the command of the animal, may be accumulated in these muscles so as to increase their susceptibility of action. In the same manner, when we direct our attention to an object, we simply render the mind more susceptible of action ; and analogy would lead us to suppose, that we produce this change in the state of the mind, in the same manner as we produce a corresponding change in the state of voluntary muscles. The brain, spinal marrow, and nerves, correspond in structure, and, therefore, we may presume that they correspond in action. Mind may be supposed to exist in acephalous animals, because the spinal marrow may be capable of imparting as much arterial or nervous influence as is necessary to support the action of their imperfect minds. I would go one step farther:—We observe and acknowledge the operation of the will in animals in which no brain or nervous system have been discovered. The imperfect minds which are given to these animals may be supported by the arterial influence derived from the circulation; that is, from the nervous influence *before* it is accumulated in the brain and nervous system. The action of the mind, in superior animals, requires a greater supply of the nervous influence than can be imparted by an imperfect nervous system, and, therefore, in the superior animals, we always observe a brain; an organ in which the arterial influence is accumulated for the purpose of supporting the action of every organ in the body, and particularly that of the mind. The brain, then, is the organ of the mind *indirectly;* in other words, the influence of the brain is necessary to the action of the mind, in the same manner as the influence of the nerves is necessary to the action of other organs. How can the brain be regarded as the exclusive organ of the mind, as the mind itself, when we observe those phenomena which indicate the agency of mind, in animals which have no brain? It is wholly an assumption to say that these animals have no consciousness. I am of opinion that those facts, which have led to the conclusion that the brain is the exclusive organ of the mind, admit of a different explanation, and that, in *all* these cases, the loss of sensation may be traced, either to a want of power in the organ upon which the impression is made, or to a want of power in the mind itself, to perceive the effect of the impression; and that this want of power, both in the organ and in the mind, is dependent upon the diseased state of the nervous system. If the mind be in a fit state to perceive the change of action, which is produced by an impression made upon any part of the body, there seems to be no necessity for that change being conveyed to the brain, because no good reason can be given, why the mind should be more capable of perceiving a change in the brain, than in any other organ. The nervous influence may be considered as the bond of connexion between mind and matter. Without the nervous influence the mind is (if I may use the expression) paralytic; it does not possess susceptibility of action.

I may appear, to some of my readers, to

have made a distinction without a difference. The fact, it may be said, that the brain is the organ of mind, still remains where it was before, because the influence of the brain is necessary to its action. But if the brain be the organ of the mind indirectly, and only in the same sense as the nerves are the organs of motion, we may reason upon affections of the mind in the same manner as we reason upon the diseases of other organs; and our views of the nervous system are not obscured by the imaginary existence of two opposite actions in one nerve, or of two opposite actions in different nerves of precisely the same structure. The nervous system is simply an organ of power imparting its influence to mind and matter in the same manner and for the same purpose.

An impression made upon any part of the body must, in order to produce sensation, produce a certain action in the organ upon which this impression is made; and the mind must be in a fit state to perceive that action. If from any cause this action do not take place, or the mind be not in a fit state to perceive it when it does take place, no sensation will be excited. How, it may be asked, does the mind perceive this action? If physiologists will inform me how the mind perceives a change in the organization of the *brain*, I will tell them how it perceives a change in any other part of the body. We say that the heart is *sensible* to the impression of its appropriate stimulus—the blood; but we know nothing of that property by which it is sensible of, by which it perceives a change in, the relative situation of its particles. We see that this change gives rise to another which we call action; and, in order to express the fact that the heart acts in obedience to its appropriate stimulus, we say that the heart is *sensible* to the impression of that stimulus. This has been denominated sensation without consciousness. We know nothing of that property which thus perceives, which is sensible of, which acts in obedience to the stimulus applied; but, in whatever manner we express the fact, we are under the necessity of using some word which is connected with an acknowledged function of the mind. Without saying that the heart possesses mind, it possesses some property so nearly allied to it, that we can form no idea of the nature of that property, which is distinct from mind, which does not bear a nearer resemblance to what we call mind than to any thing else. This property, whatever it may be, is called into action by a change in the state of the organ;

and therefore this change in the state of the organ may be denominated the stimulus to that action which we denominate sensation without consciousness. In the same manner, a certain change in the organization of living matter acts as a stimulus to that power, or property, or imaginary organ, which we denominate mind. The process of sensation, then, appears to be as follows:—An impression made upon an organ produces a change in the relative situation of its particles, and this change acts as a stimulus to the mind, by which that mental action, which we denominate sensation, is excited. If the change do not take place, the mind is inactive for want of its stimulus; if the mind be not in a fit state to be acted upon, the change may take place and yet not lead to sensation, because it will not produce a corresponding change in the mind; and the mind will not be in a fit state to be acted upon, unless it be supplied with that portion of the nervous influence which, in the superior animals, is necessary to its irritability or susceptibility of action. The nervous influence is necessary to the existence of that action which is the stimulus of the mind, and to that state of the mind which constitutes its capability of being excited into action by the stimulus applied.

Now the action of the mind may, in the superior animals, be supported by the brain particularly or altogether, and therefore, every action of the mind is attended with a consciousness of *that part of the nervous system* being called into action, and this sensation has induced us to regard the brain as the organ of the mind; but, in reality, the brain is no more the organ of the mind, than the nerves of voluntary muscles are the organs of motion. The brain and the nerves supply power to their appropriate organs, and the effects produced, viz. sensation and motion, are the actions of the mind and of muscular fibre, and not of the brain or nerves. Dr. Currie says, "As *madness* is, I think, best combated in the height of phrenzy, so, I believe, *convulsive diseases* are most capable of remedy in the agitation of convulsion. I seem to have *a glimpse of a general principle in nature that connects facts so apparently different*, but the ground is not yet firm enough for the foundation of a theory."* If the diseased actions of the mind and of the body so nearly resemble each other, as to yield to the same principle of treatment, is it not reasonable to suppose, that the healthy actions of the mind and of the body are regulated by the same laws, and that the actions of the mind and of the voluntary muscles may be equally dependent upon a due supply of the nervous influence?

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* Currie's Reports.

We learn from experience and observation that a paralytic muscle is not always devoid of sensation; in other words, it has been observed that a muscle may lose its susceptibility of motion, and retain its susceptibility of sensation. The simple explanation of this fact would appear to be, that the nerves of sensation and of motion are distinct; that there are two sets of nerves, one contributing to sensation and the other to motion; and it has been assumed as a fact, that the former act from their extremities to the brain, and the latter from the brain to their extremities. If the nerves of sensation and of motion could be traced to different parts of the brain, so that the injury of one part of the brain might affect the nerves of sensation, and the injury of another part of the brain might affect the nerves of motion, the idea would be deserving of credit; but even then it would be difficult to conceive how nerves, *precisely similar in structure*, should have so diametrically opposite a function to perform, as that of conveying the influence of the will from the brain to their extremities, and the influence of a mechanical or chemical impression from their extremities to the brain. It is unnecessary to enter into the different explanations of this fact, which have been suggested by physiologists from the time of Willis to the present, because these explanations have all fallen into discredit, and have been displaced by the revived opinion, that the nerves of sensation and of motion are distinct, distinct in their origin and function, *but not distinct* in their organization. Now although this idea may *appear* to offer a ready explanation of the fact, that a muscle may lose its susceptibility of motion, and retain its susceptibility of sensation,* it involves another difficulty of equal magnitude. How do parts of the body which are insensible in health become sensible in disease? We cannot suppose that nerves of sensation exist in health without any function to perform, or that they are generated by disease for no good purpose. I am disposed to think that the experiments of Messrs. Rolando, Fleurens, Dr. W. Philip, Magendie, and Bell, have led to conclusions upon this subject which do not legitimately arise from the experiments themselves. The experiments of the latter author have been supposed to *establish* the fact, that the nerves of sensation and of motion are distinct in their origin. "The nerves of the spine, the tenth or sub-occipital nerve, and the fifth or trigeminus of Willis, constitute this original and symmetrical system. All these nerves agree in these essential circumstances: they have all double origins; they have all ganglia at one of their roots; they go out laterally to certain divisions of the body; they do not interfere to unite the divisions of the frame; they are all *muscular* nerves, ordering the *voluntary motions* of the frame; they are all exquisitely *sensible*, and the source of the common sensibility of the surfaces of the body; when accurately represented on paper, they are seen to pervade every part; no part is without them; and yet they are symmetrical and simple as the nerves of the lower animals."* These nerves, then, are nerves both of sensation and of motion, and are supposed to be endowed with this double property in consequence of their double origin. Each distinct nerve has a double origin; in other words, two nervous fibrils, originating in distinct parts of the spinal marrow, unite into one nerve. This is the necessary interpretation of the passage, because, if the nervous fibrils did not unite, they would be simply distinct nerves with a single origin. This single nerve is supposed to derive the nervous influence from the spinal marrow by one fibril, and to communicate a certain impression or action to the spinal marrow by the other fibril; that is, the action of one fibril is from the spinal marrow to the nerve, and of the other fibril from the nerve to the spinal marrow, and thus a single nerve becomes the organ of two functions. But this explanation does not appear to me to lessen the great difficulty with which the physiology of the nerves has had to contend; because the action of *the nerve itself* is still considered to be progressive and retrograde, progressive from one fibril and retrograde to the other; because we attribute to the two fibrils, (the organization of which appears to be similar,) two opposite functions; and even in this we are not consistent, because the action of the nerve itself is *both* progressive *and* retrograde, whereas the action of the nervous fibril is simply progressive or retrograde; because we attribute to different portions of the spinal marrow a directly opposite function. It has been said, that if the *anterior* root of one of these nerves be divided, the muscle which that nerve supplies becomes paralytic, and if the *posterior* root be divided, the muscle which that nerve supplies loses all sensation. That the muscle should become paralytic, in consequence of the anterior nervous fibril being divided, is exactly what might have been expected, because, from the diseased state of the nerve the animal cannot concentrate in the muscle as much nervous influence as is necessary to its action; I mean that action which is capable of being perceived, and which we denominate its contraction, in other words, muscular motion; but the nerve in its imperfect state may supply as much nervous influence as is necessary to that invisible action, which is the immediate stimulus of the mind, and leads to sensation; if both roots be divided, the muscle is deprived of both sensation and motion, because it is deprived of *all* action, without which neither sensation nor motion can take place. That the division of the *posterior root only*, will leave the organ which it supplies subject to the will of the animal, and at the same time insensible to any impression which can be made upon it, I am much disposed to question. How is it possible to ascertain the sensations of the animal? If we consider *the state* of the animal when the operator is dividing, or has divided, the posterior root of a spinal nerve, I am disposed to think that the opinions of the operator as to the *sensations* of that animal may be liable to some little error. The operation of dividing a nerve in a more superficial part of the body may be less liable to error from the circumstances alluded to. Thus we are told " that if the fifth nerve and portio dura of the seventh be both exposed in the face of a living animal, not the slightest doubt will remain in the mind of the operator as to which of the nerves bestows sensibility. On the nerve of this original class being divided, the skin and common substance are deprived of sensibility; but a nerve *not* of this class being divided, it in no measure deprives the parts of their sensibility to external impressions." Is it clear that both these nerves supply the same part of the same organ? May not the function of the two nerves be the same, and the loss of sensation or of motion be dependent, not upon any indifference in the nerves themselves, but upon the organization which those nerves supply? "On the *division* of the portio dura on one side of the head of an ass, *he gave no sign of pain*; there was no struggle or effort while *cutting across this* nerve, and the animal ate corn and hay as usual; but on *touching* the superior maxillary branch of the fifth pair, there was acute pain." I have already alluded to the fallacy of any opinions which may be formed as to the sensations of an animal; I shall now point out a source of fallacy in the operation itself. There was no struggle while *dividing* or *cutting across* the portio dura, but on *touching* the superior maxillary branch of the fifth pair, there was acute pain. The most effectual mode of preventing pain from a nerve is that of *dividing* it. That nervous matter is not sensible, there are many reasons for believing. Now, by dividing, by destroying the organization of a nerve, we destroy its power of action which we have stated to reside in the neurilema. No sensation, therefore, was experienced, because the nerve, *when divided*, was incapable of that action which is necessary to sensation. By *touching* a nerve, we do not destroy the organization; we irritate the active part of the nerve; we excite the *action* of the neurilema, and consequently produce sensation. The difference between dividing and irritating a nerve is too apparent to require any comment. Can the portio dura be *irritated* without exciting any sensation in the animal? We are told that the slightest touch of the portio dura, or respiratory nerve, convulsed the muscles of the face, yet *still he gave no sign of pain*. The portio dura, I may observe, has, from its peculiarity of structure, acquired its peculiar name. It may, from the peculiarity of structure be *less* susceptible of action and consequently of sensation than other nerves; and, therefore, the conclusions drawn from experiments upon this nerve may not be strictly applicable to nerves of a different structure. And what is the fact, upon which a conclusion so important in its consequences and applications, is founded? "*Yet still he gave no sign of pain.*" Is the author justified, upon such evidence, in assuming the fact that the animal positively suffered no pain? "The cutting of the fifth nerve *gave pain* in a degree corresponding with our notions of the sensibility of nerves." How was the cutting performed? The operator can only form his idea of the pain excited by noticing the *actions* of the animal. In order to demonstrate those actions he must *delay* the operation, and thus not only divide but irritate the nerve. Can we, upon evidence which admits of being questioned in every item, which is so connected with the mode of performing the operation, and which is altogether founded upon the ideas of the operator as to the sensations of the animal, consider the important fact to be established, that the nerves of sensation and of motion are distinct in their origin and function? Admitting the correctness of the author's ideas as to the sensations of the animal, and as to the different effects produced by the division of different nerves, it is by no means clear that these different nerves were distributed upon the same organization, upon the same part of the same organ; and thus the difference of effect might depend, not upon the nerves themselves, but upon the peculiar organization which these nerves supply. Admitting the fact to be fully established, we are still unable to say, why a paralytic muscle retains sensation, because we are unable to say why the nerves of motion are diseased, and the nerves of sensation are not; and the theory is absolutely inconsistent with the acknowledged fact, that parts insentient in health become acutely sentient in disease. How then shall we explain the fact that a paralytic limb often retains sensation? Instead of looking to the nerves alone for the explanation of this fact, let us direct our attention to the difference in the organization of the muscle itself. In consequence of some affection of the brain or nervous system, the animal is not capable of accumulating in a paralytic muscle as much nervous influence as is necessary to its action; that is, as much nervous influence as will raise the irritability of a muscle to that point which will render the stimulus of distension, (always present in an attached muscle) capable of exciting its visible contraction; but the stimulus of a foreign body may be sufficient to excite in a muscle (which contains but a small portion of the nervous influence, and which portion may be derived from its nerves or even the arterial system) that invisible action which is the immediate stimulus of the mind and leads to sensation; and thus the difference between the perceptible effect produced by the contraction of a muscle, which we call motion, and the imperceptible action which leads to sensation, may be dependent upon the difference in the state of the muscle itself as connected with the diminished supply of the nervous influence, and the consequently diminished irritability of the organ itself. In those cases of paralysis in which the power of voluntary motion exists and sensation is lost, I am disposed to think that the loss of sensation must be referred to the skin and not to the muscle. Every organ, then, which is capable of action, may become an organ of sensation, because no good reason can be given why the mind should be more capable of observing a change in one organ than in another; and thus no retrograde action of the nerve is necessary to sensation. The peculiar and only function of the nerve is that of communicating its influence to other organs; but the action of the nerve by which that influence is supplied, may, like the actions of other organs, become a stimulus to the mind, and lead to sensation. The papillæ of the skin may be the peculiar organs of touch; and sensation may depend, not upon the nerves which supply those organs, but upon the actions of the papillæ themselves.

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* The nerves of sensation and of motion are supposed to emerge from the whole course of the spinal marrow alternately, the former deriving their origin from a strip of medullary matter which passes down the internal surface of the spinal cord, and the latter from a similar structure on the external surface. The nerves of sensation and of motion being so distinct in their origin, how does it happen that a muscle frequently, I might *almost* say universally, in cases of paralysis, loses its power of motion and retains its power of sensation, and that the loss of sensation, without the loss of motion, is, comparatively speaking, a very rare occurrence? Why is one part of the spinal marrow so constantly affected by disease, and not the other?

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* Philosoph. Transact. 1821.

The skin itself may be the organ of touch, and capable of being excited into that peculiar action which leads to sensation. The papillæ of the tongue may be the organs of taste, the schneiderian membrane of smell, and the retina of vision; or these peculiar senses may depend upon an action of the nerves themselves.

Any discussion upon this subject is wholly unnecessary, because I do not deny that nerves are organs of sensation, but I believe that all living organization, which is capable of being excited *into action*, is capable of becoming the medium of sensation.

I shall defer any observation I may have to make upon the different senses, and upon the actions of the mind itself, until I come to treat of their pathology; but there is a state of mental inaction so nearly allied to the health of the system, that I cannot pass it over unnoticed. I allude to that state of mental inaction which we denominate sleep. The action of the mind is dependent upon a due supply of the nervous influence, by which susceptibility of mental action is acquired, and upon the application of certain stimuli by which that action is excited. The susceptibility of mental action is gradually diminishing through the day, in consequence of the supply of nervous influence being less than the expenditure. This state of the mind is accompanied by a sensation which we term languor, or a disposition to sleep; if, under these circumstances, the stimuli of the mind be industriously avoided, there are two concurrent causes of mental inaction; the mind is less disposed to be acted upon by its appropriate stimuli, and these stimuli are diminished. If we regard the action of the mind as dependent upon a due supply of the nervous influence, and upon the application of its appropriate stimuli, we have no difficulty in accounting for the sleep of apoplexy, the sleep of fainting, and the sleep of death.

The sympathy of different organs has been attributed to nervous communication; but as parts sympathize which have no particular nervous communication, and others, between which this communication may readily be traced, do not sympathise, considerable doubts may be entertained as to the correctness of that theory, which attributes the sympathy of different parts to the agency of nerves. By referring all sympathy to the brain as a central point, we do not at all lessen the difficulty, because the circumstance of all parts of the body communicating directly or indirectly with the brain, does not explain why certain parts only sympathize with each other. This subject will be adverted to hereafter.

I shall now briefly recapitulate some of the leading points which have been suggested to my reader, and proceed to the consideration of the pathology, or diseased action, of the system. Life is a certain unknown principle superadded to organization, wholly independent of matter for its existence, but altogether dependent upon matter for its union with the body.—The only function of life is to preserve the organization of matter.—Life and living action are not the same things, the former being independent of matter for its existence, the latter being a property of matter itself; the agency of the former being to counteract, and of the latter to promote, a change in the organization of matter.—All living action is motion—all living action is dependent upon, and regulated by, the state of the organization.—The immediate stimulus to every action is a change in the relative situation of the organic particles. Some organs, as the voluntary muscles, are always in a state of tension, and, therefore, always possess the stimulus to action in themselves.

The action itself is a counteracting change in the relative situation of the organic particles, by which the effect of the stimulus is counteracted, and the organic particles contract upon themselves.—The power of an organ is that property upon which the counteracting change or re-action is dependent, and corresponds with the nature of the organization.—The immediate effect of living action is to accelerate the disorganization of the body, and more particularly the expenditure of the aërial influence; but the support and renewal of the organization are the effects of the combined actions of the system. Thus the action of an individual organ leads to its own disorganization, but at the same time contributes to the support of the organization generally. The action of the stomach, for instance, has a tendency to destroy its own organization, but at the same time produces that change in the food, which renders it capable of being assimilated with the system generally, or with itself individually, through the medium of other organs. The action of the heart, in its immediate effect, has a tendency to destroy its own organization, but at the same time gives an impulse to the blood, by which that fluid is sent to the system generally, and to itself individually, for the purpose of being assimilated with the body through the medium of other organs; and thus the action of an organ contributes, at the same time, to the organization and to the disorganization of its own structure. A constant change in the organization of matter is the effect of this law, which is peculiar to the living system, which defies all imitation, and which has *ignorantly* been attributed to chance.

The actions of the body may be arranged according to their *effects*, and reduced to three classes, the locomotive, the digestive, and the sensitive functions.*—The air may be considered as the food of the body ; it must be digested and assimilated with the system, before it can answer the purposes of the animal economy ; it is, when digested, the principle upon which living action is more immediately dependent ; when united with the blood it constitutes the arterial influence, and when accumulated in the nervous system is denominated the nervous influence ; it is accumulated in the nervous system, for the purpose of contributing to the support of living organization and action, according to the will or to the necessities of the animal ; it is the source of animal heat, not in consequence of the change which it undergoes in the lungs, but of the change which it undergoes in every part of the system ; it contributes to the support of corporeal and mental action, by supporting the irritability both of the body and of the mind.—The nervous system may be regarded as a secondary circulating system, by which the arterial influence is conveyed to the several organs of the body.

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* The locomotive and sensitive functions may appear to correspond with the animal life, and the digestive functions with the organic life of Bichât ; but the principle of arrangement is different, and does not involve the visionary idea of two distinct systems in one body.

—The ganglia, spinal marrow, and brain, may be considered as so many reservoirs of the arterial influence.—The action of nerves and of arteries appears to be the same.—Nerves are not the *exclusive* organs of sensation. The brain is the organ of the mind indirectly; it contributes to the support of mental action, in the same manner as nerves contribute to the support of voluntary motion; and lastly, the action of the nervous system is always from the brain to the extremities of nerves, and *never* from the extremities of nerves to the brain.

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